

**THE ROUTES OF SENSE  
THOUGHT, SEMANTIC UNDERDETERMINACY AND  
COMPOSITIONALITY**

**Walter B. Pedriali**

**A Thesis Submitted for the Degree of PhD  
at the  
University of St Andrews**



**2012**

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# **The Routes of Sense**

**Thought, Semantic Underdeterminacy and Compositionality**

Walter B. Pedriali

A thesis to be submitted to the  
University of St Andrews  
for the degree of  
Doctor of Philosophy

Departments of Philosophy  
School Philosophical, Anthropological and Film Studies  
University of St Andrews

November 2011



I, Walter B. Pedriali, hereby certify that this thesis, which is approximately 80,000 words in length, has been written by me, that it is the record of work carried out by me, and that it has not been submitted in any previous application for a higher degree.

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## Abstract

What does it mean to be a rational language user? What is it to obey linguistic rules? What is the proper account of linguistic competence?

A Fregean answer to these questions would make essential appeal to the notion of sense: we are masters of a language to the extent that we are able to recognise the cognitive value of its expressions; we are rational judges regarding truth-value assignments to the extent that we are sensitive to the ways in which the sense of an expression guides us in the semantic evaluation process; and as for obeying rules, it is our ability to respond to how sense directs us, for a particular assertion of a sentence, towards the determination of its truth-value that best exemplifies what it is like to follow a linguistic rule.

My thesis explores a cluster of closely interrelated issues arising from these questions (whether or not considered from a Fregean perspective).

Accordingly, in tracing the routes of sense my dissertation places itself at the intersection of the philosophy of language, linguistics, philosophy of logic, and meta-ethics—and indeed, I end up agreeing with Allan Gibbard that the theory of meaning really belongs to meta-ethical reflection.

Chapter 1 introduces some of the main research questions that I try to address in the rest of the thesis.

In chapter 2 I state a number of theses which I take to be the defining ones for semanticism. I show that they form a class of jointly incompatible commitments. I choose nonsense as a problem case for compositionality and I argue that it forces the semanticist to abandon either the learnability or the compositionality constraint. The escape route I adopt, going radically minimalist about content, is incompatible with another key semanticist thesis, namely, that grasp of meaning is grasp of truth-conditions (robustly conceived).

In chapter 3, I consider the account of atomic meanings given by both the semanticist and the pragmaticist and I conclude that on neither account does interpretation come out as a process of rational

choice between candidate bearers of content. Again, I suggest the lesson from indeterminacy is that we ought to embrace an ineradicably minimal conception of content.

In chapter 4 I turn my attention to the meaning of the logical constants and I argue that indeterminacy worries extend to the very heart of the compositional machinery.

Chapter 5 examines the view that logic is the science of reasoning. Unsurprisingly, I conclude that a defence of this claim requires endorsing content-minimalism.

In chapter 6 I conclude my dissertation by sketching a radical view of content minimalism and I try to show how it can solve the puzzles I had been considering over the course of the previous chapters.

## Acknowledgements

The idea (and the title) for this dissertation came to me while reading Dummett (1990a: 227), a short, highly compressed review note on David Bell's book on Husserl. The crucial question discussed in that note was the relationship between the Fregean notion of sense (for Dummett, an essentially object-directed one) and Husserl's views regarding objectual reference.

Dummett pointed out that in giving an account of intentionality and meaning, we must do justice to both the intensional (object-independent) and the extensional (object-dependent) mode. The difficulty is becoming clear as to which mode Fregean *Sinn* belongs to.

In stating his own view of the matter, Dummett said the following (p. 227):

We must not [...] think of a Fregean sense as an intermediate station *en route* to the referent, as if the thinker aimed at the sense, which then readdressed the thought to the referent. The sense is itself the route; the entire route, and nothing but the route.

At that point, I knew I had my title.

In the next page, Dummett went on to say that

everything that goes to determine the referent is part of the sense. We apprehend the referent *through* the sense; the sense simply is a way of conceiving of the referent

At that point, I *thought* I had the topic for my thesis: the routes of sense are all the ways for *everything* that fixes reference to be cognitively accessible to us (the full significance of that quantifier was to become clear only much later).

In my year studying for an MSc at Manchester, I was exposed to a multitude of logics, mostly of the constructive variety, and I started to think that they could provide the perfect technical tools to implement Dummett's idea. Dalrymple *et al.* (1995) and Kempson *et al.* (2001)

in particular seemed to have exactly the sort of toolkit I needed for the job.

In September 2006 I started my PhD with a firm plan of action on hand, or so I thought. Then I read Travis' *The Uses of Sense and Thought's Footing* and my confidence started to wobble.

By the end of my first year as a PhD student I realised two things: that life in a busy research centre like Arché meant that a project like mine—one, that is, that was not just wildly ambitious but also not directly aligned with any specific project within the hectic life of the centre—would take longer than three years to complete; and that my once robust sympathies for formalist approaches to natural language semantics had given way to an appreciation of the seriousness of the Wittgensteinian challenge to those approaches.

The project grew in breadth and scope, encompassing more and more issues to do with rational language use—making us rational in Frege's puzzle situations was after all precisely the motivating thought behind the very notion of *Sinn* that I was trying to stabilise.

Five years on, the project is finally done. Or rather, it has reached a stage where the requirements of a *publish and be damned* deadline can no longer be ignored.

And in the end, the view I defend in this thesis is one whereby the intensional and extensional modes join up in a drastically internalist, radically minimal view of content (both semantic and mental) whereby our thinking inhabits a world of senses that are not Fregean in character (they are not determinately configured in the manner assumed by Frege) and that gain whatever traction (and determinacy) they gain only through our environmental embedment (that's where Dummett's *everything* gets to do a lot more work than previously expected).

The structures that the language faculty generates, that is, are in my view even more abstract than Chomsky thought: they are indeed the routes that sense takes to connect us to the world but they remain highly indeterminate (and ineradicably skeletal) throughout our dealings with the world.

In letting go of my thesis, I have a list of people I should thank for helping me out along the way.

Peter Clark provided help and encouragement since my earliest days as an undergraduate at St Andrews and it is hard to imagine that I could have begun, prosecuted and completed my thesis without his watchful presence for the past ten years.

For the first three years of my PhD, I was in receipt of an AHRC doctoral award (2006/125079) without which this thesis would not

have been possible. The Department at St Andrews also provided me with a three-year St Andrews scholarship for which I remain deeply grateful.

In my fourth year, I followed my then first supervisor, Professor Crispin Wright, to Aberdeen and while there as a Research and Teaching Fellow I was also fortunate enough to be awarded a scholarship by the College of Arts and Social Sciences which I gratefully acknowledge.

It is a matter of considerable regret that circumstances made it impossible for me to terminate my studies under Crispin's guidance. His crucial contributions to the old debate on the theory of meaning back in the 1980s provided the starting point (and much of the motivation) for my thesis and I often felt that I was trying to pick up the strands left open from that debate—indeed, this dissertation should be seen as an attempt to answer the research question left dangling at the end of Wright (1981).

My return to St Andrews was not without obstacle and it is my pleasure to record my eternal gratitude to Derek Ball, Jessica Brown, Herman Cappelen, Patrick Greenough and Stephen Read for their invaluable help in making it at all possible.

Among my Arché fellow students, I must single out Ralf Bader, Dirk Kindermann, Guðmundur Andri Hjálmarsson, Ole Hjortland, Torfinn Huvenes, Anders Schoubye and Andreas Stokke for their very helpful feedback, encouragement and advice.

My progress reviewers over the years, namely, Katherine Hawley, Martin Smith, Peter Clark, and Elia Zardini, have also been extremely helpful and I extend my thanks to them here.

Material that ended up in chapter 2 was first presented at the St Andrews Friday Seminar in April 2009. Andreas Stokke was the respondent and his input helped put the nonsense problem into sharper focus. Edward Witherspoon, then visiting from Colgate, also provided highly fruitful discussion of Wittgenstein's conception of nonsense.

Much of chapter 4 was discussed at two FLC meetings and I am grateful for feedback to Colin Caret, Ole Hjortland, Torfinn Huvenes, Stephen Read, Anders Schoubye and Elia Zardini.

The first few sections of chapter 5 formed the basis for talks at the FLC workshop in March 2010 and at the Brown Bag seminar series at the UConn Philosophy department, during a three-week visit organised by Sharon Coull and Marcus Rossberg. During my stay in that wonderful department, I had the opportunity to discuss the core arguments in my thesis with Sam Wheeler, whose book on analytic philosophy and deconstruction first alerted me to the points of con-

tact between the work of Davidson, Quine and Derrida. Sam's generous and insightful input was very helpful indeed and provided much needed encouragement. Discussion with Marcus and Patrick Greenough, then also a visitor at UConn, further helped. Among the audience at my UConn talk, I am grateful for feedback to JC Beall, Paul Bloomfield, Crawford Elder, Joel Kupperman, Michael Lynch and Ruth Millikan.

In August that year, I presented material that ended up in chapter 6 at the CSMN Conference on Response-Dependent Concepts, Oslo. Thanks are again due to Sharon for organising my trip and to the audience there.

Before closing, a few people deserve a special vote of thanks.

When Arché split in 2009, I experienced a most traumatic, heavily disruptive time. Andreas Stokke provided a much appreciated and highly sympathetic ear until my return to St Andrews.

Throughout that time, and indeed to this day, Herman Cappelen has shown tremendous kindness and understanding without which I do not think I could have completed this thesis. I am also most grateful to him for having accepted to be my internal.

It is a matter of great joy that Agustín Rayo is my external. I was an undergraduate when he arrived at Arché and he immediately struck me as the sort of philosopher I wanted to become. No doubt it will be a matter of considerable bewilderment to him to see the sort of philosopher that I *have* become.

And going back to my undergraduate years, without Philip Ebert's encouragement I would not have gone on to postgraduate studies. I am still not sure whether I ought to be grateful to Philip for that, but his friendship has meant a lot to me over the years, as has that of his Frege twin Marcus.

Derek Ball has provided very detailed comments on the early chapters of the thesis which I have done my best to incorporate.

Stephen Read was my first supervisor and helped me define my research proposal before I started my PhD. Stephen has taught me (or tried to) much of the stuff that forms the basis of this thesis. I am sure he was disappointed when my project steered away from his proof-theoretic interests and I can only repeat my apologies for that here. With characteristic generosity, Stephen put tremendous effort in getting me back to St Andrews. My thesis is dedicated to him for all his help over the years.

Patrick Greenough stepped in as my main supervisor in January 2011. It is fair to say that until I started working with Patrick I did not have a thesis. I probably had three or four theses (at various stages

of inarticulacy) stacked away in my drawer, but no single unified line. Patrick's quietly penetrating, confidence-inducing remarks prompted a highly overdue change of gear. I wrote the entire thesis afresh for him, with him in mind as my ideal reader, that is. I think I finally found a new voice under his gentle but firm prompting. He's been truly the perfect catalyst for the entire project and it is hard to put my gratitude into words.





## Chapter 1

# Rooting for Sense

### 1.1. Introduction

Why is the theory of meaning so *hard*?  
Hilary Putnam<sup>1</sup>

Putnam once posed the question whether semantics was so much as possible. His answer was a qualified one. Yes, we can still do semantics, but only if we come to accept that natural language semantics is a sloppy, impressionistic discipline. Its lack of precision and of mathematical rigour should not surprise us, he added: it is, after all, “a typical social science.”<sup>2</sup> According to Putnam, the grand, formalist tradition had been guilty of oversimplification—roughly: that the technical achievements in formal logic could be transposed without residue to the field of natural language (NL) studies.<sup>3</sup>

Despite Putnam’s misgivings, NL semantics continues to be a thriving area of formal research and, forty years on from his warning, philosophers are still busily engaged in the attempt to give systematic, fully rigorous accounts of linguistic phenomena in our vernacular.<sup>4</sup>

No-one denies that there are social aspects to language: but for the formal semanticist there is a core class of truths regarding linguistic structure that largely transcend the contingencies of our practices. These truths hold universally (in the sense that all possible languages

<sup>1</sup>Putnam 1970: 139.

<sup>2</sup>Ibid.: 152.

<sup>3</sup>A similar judgement, but from a different perspective, is drawn in Barwise and Perry (1983: ix). To put it provocatively: I would reverse Putnam’s judgement and argue that considerations arising from a proper examination of NL should influence the way we view those technical achievements.

<sup>4</sup>The quest for systematicity extends to cases where one would not *prima facie* expect to find it. See for instance the (exciting) discussion of the semantics of ‘wh- the hell’ expressions in Dever (2006: §26.13).

respect them) and they provide the key to the proper explanation of linguistic competence.<sup>5</sup>

## 1.2. The Extension Problem

Now, as I see it, the crucial issue in semantics is the *Extension Problem*, the problem of explaining how the semantics for a class of expressions can be extended to a semantics that covers other, suitably related classes of expressions. For example, suppose we are given a semantics for the atoms in the language; then the extension problem becomes that of deriving a semantics for the complex expressions that somehow *respects* the atomic meanings (and analogously for the opposite direction).<sup>6</sup>

Clearly, the Extension Problem underpins the main (conceptual) issue that semantics is standardly asked to confront (and explain), namely, that of how we understand novel complex expressions.

For Putnam, however, the most pressing question was its converse: how we come to understand novel atoms.<sup>7</sup> The difference is important and it does indeed separate formal semanticists (henceforth ‘semanticists’) from pragmaticists.<sup>8</sup>

In essence, the semanticist’s claim is that we understand novel complex expressions in virtue of our grasp of their syntactico-semantic structure.<sup>9</sup>

Given that the atoms lack (compositionally derived) structure,<sup>10</sup>

<sup>5</sup>For some claims along these lines within linguistics see e.g. Chomsky’s (1995a: 385) insistence that ‘considerations of virtual conceptual necessity’ impose specific conditions on the human language faculty. For more talk of necessity see also Culicover (1997: 4), Newmeyer (2005: 8), Boeckx (2006: 10), Hinzen (2007: 27), Moro (2008: 5–6), Hornstein (2009: 18). On the philosophers’ side, see e.g. Lepore and Ludwig’s (2007: 1) claim that a theory of meaning “seeks to understand meaning at a level that abstracts away from contingent features of human languages”. For linguists, the crucial facts concern phenomena such as dependence and locality constraints. For philosophers, compositionality is the central requirement on a semantics. This is my main concern in what follows.

<sup>6</sup>Roughly, the problem is how to prime the atoms and the compositional rules so that every combinatorial possibility is mapped (and classified) ahead of use in a manner that delivers the intuitive truth-conditions for NL sentences. Details in ch. 2.

<sup>7</sup>See Putnam (1970: 149).

<sup>8</sup>Under the label ‘semanticist’ I intend to gather all those theorists who hold that (syntactic) structure determines (fully controls) semantic content—e.g. Stanley (2007). Their opponents are standardly termed ‘pragmatists’ but following Levinson (2000) I prefer to use ‘pragmaticists’. See also fn. 17, p. 16.

<sup>9</sup>Henceforth, bare reference to structure will be taken to mean ‘syntactico-semantic structure’.

<sup>10</sup>At least on most views: for one recent exception see Hinzen (2007: ch. 5).

those who think that the fact crying out for (semantical) explanation is our grasp of the atoms will tend to place themselves at the opposite (pragmaticist) end of the spectrum, since an explanation of our competence in terms of apprehension of structural facts will not be readily available in the case of atoms.<sup>11</sup>

The contrast then is between theorists for whom our linguistic knowledge *essentially* rests on grasp of structure and those for whom it does not.<sup>12</sup>

The Extension Problem is my main topic in what follows. As we shall see in the chapters ahead, it gives rise to several deep puzzles. And it seems to me that, in thinking about semantics, this is *the* problem we should worry about, for it brings our theoretical commitments into sharp focus, indeed it tests them to the limit. It is thus the starting point to semantical and meta-semantical investigation, since our ability to effect the *transition* from atoms to complexes (and the linguistic judgements regarding meaningfulness and grammaticality that we make based on that ability) is both the main phenomenon to be explained and the chief motivation for some of the key assumptions made within a semanticist framework.

Indeed, where theorists will most differ is precisely with respect to the proper account to be given of our ability to move between the lexicon and the set of complex expressions. For the semanticist, that ability only makes sense if we appeal to grasp of structure; not so for the pragmaticist.

More importantly still, the proper solution to the Extension Problem will dictate constraints on our conception of content, both semantic and mental.

The question the problem brings into sharp focus, that is, concerns *what it is* that we understand when do we grasp meanings (both atomic and complex). And indeed whether semantics is so much as possible (and the extent to which it *is* sloppy) will largely depend on the cognitive profile of the objects of linguistic competence (and, relatedly, of our propositional attitudes more generally).

Let me now introduce some of the puzzles before us.

<sup>11</sup>Note that some pragmaticists will insist that even in the case of structurally rich expressions our understanding will not necessarily depend on (or be exhausted by) grasp of structure.

<sup>12</sup>One might argue that grasp of atoms and grasp of complex expressions are *different* phenomena, mobilising different abilities. It might then seem as if the controversy here is purely (or largely) terminological. I do not think so. The shift in emphasis across the two camps reflect contrasting (and indeed incompatible) claims about the precise shape (and *reach*) of our linguistic competence.

### 1.3. Projecting Meanings

Endowing words with meaning is something *we* do. And we are also rather good at *recovering* meanings from words.<sup>13</sup> Moreover, our meaning-recovery ability seems appropriately *unbound* and for the most part a matter of routine.

On reflection, this is (or ought to be) puzzling on a number of counts. If meaning originates with us—i.e. with our choices and conventions—it may appear mysterious *that* words can acquire an independent life, as it were, and stably preserve their meanings across a (seemingly) open-ended multiplicity of sentences which couldn't possibly have been incorporated in the original act of meaning-fixing.<sup>14</sup>

Or at the very least: it may (and should) appear mysterious *how* such a feat of forward-looking meaning-determination could be accomplished—how could *anyone* predict, from an act of context-bound meaning-conferral on a word, *all the facts* about its semantic behaviour under embedment in an unbound class of as yet undisclosed linguistic and non-linguistic contexts?

Equally, it may (and ought to) seem just as mysterious how our acts of meaning-fixing can successfully *guide* future applications of the language's expressions; that is, the putative recovery of meanings *under precise normative constraints* in what looks like a virtually inexhaustible variety of (linguistic and worldly) contexts is no less perplexing than their forward-looking determination.<sup>15</sup>

If genuine, the puzzle, then, concerns both the (relative) *stability* of meaning amidst the contextual whirlwind as well as the transmission (and/or preservation) of (perhaps modestly) normative constraints on the use of expressions from the initial grasp of a patently limited lexico-syntactic basis to the (apparently derivative) grasp of a (seemingly) open-ended totality of linguistic expressions—with those constraints flowing, or so it would seem, from the purported claims of

<sup>13</sup>For brevity's sake, I am presenting as a given this simplistic picture that assumes a straightforward, unproblematic same-content connection between meaning-endowment and meaning-recovery. There are dissenting voices, e.g. Gauker (2003), Barker (2004).

<sup>14</sup>Chomsky (2009: 20) draws an intriguing parallel between Humean arguments (pretty much along these lines) for the positing of a *natural* "grammar of moral judgements" and his own nativist conception of universal grammar. We *can't help* project meanings the way we do.

<sup>15</sup>After all, the original baptism took place in a *specific* context (or a range thereof). On the semanticist story, this was abstracted (and generalised) to a *standing meaning* level. But which features of meaning do we help ourselves to when we recover the meaning-in-context? The general or the particular? See Taylor (1989: §8.4), Evans (2009: 23ff.) and Putnam (1986: 292) for discussion.

semantic stability.<sup>16</sup>

On further reflection, we could perhaps be led to deny that these intuitively compelling considerations carry much weight. It is a delusion that we do really have such powers, the destabilising thought goes.<sup>17</sup> What an expression means is entirely (more cautiously: largely) a function of our use at any given time: we *decide* (more cautiously: evaluate) every time afresh what we are going to do with a certain expression in a certain context.<sup>18</sup> No meaning-conferring act could possibly have the scope and all-encompassing authority posited in the picture above.

Meaning, on this view, is profoundly, ineradicably *unstable*. It is also inherently *local*, inextricably confined, that is, to the *particular*, contrary to the (standard) conception of meanings as entities with global powers, abstracted from particular instances of use to full, unrestricted generality—a conception that seems to underlie the very enterprise of formal semantics.

Suppose however that this deeply subversive thought has not yet emerged. In our as yet undisturbed semantic paradise, an attractive explanation of our initial puzzle seems to lie ready at hand: we can assign and recover meanings from linguistic items, the (friendlier) suggestion goes, because the meaning of a complex expression is (solely) a function of that of its sub-components and their mode of composition.

In other words, by fixing the meanings of the lexical items (roughly: by employing axioms that fix their reference) and by specifying the output of the sentential connectives via appropriate phrasal axioms, we have thereby fixed the meaning of *any* expression recursively formed from that base.

Grasp of complex meanings is thus grasp of *compositional* meaning. *This* is what solves the Extension Problem, the semanticist tells us; what's more, it is the *only* solution that can make languages *learnable* by beings like us.

<sup>16</sup>Note that the puzzle arises at a purely technical level (how can we *define* meaning functions for all expressions in the language), at a conceptual level (what is the order of determination of meanings? Is it bottom-up, i.e. from the atomic to the complex, or top-down?) and at the normative level (whence the authority of language over its practitioners?).

<sup>17</sup>Baker and Hacker (1984: ch. 9) articulate this thought in the form of a long invective against the generative enterprise. While I'm a lot more sympathetic to attempts to systematise language mastery than they are, I'm still impressed by the real difficulties in this area that their chapter highlights.

<sup>18</sup>The thought canvassed in this paragraph is obviously Wittgensteinian in character. Rayo (2008: 330) proposes a kind of hybrid view in which "linguistic practice involves semi-principled decision-making".

Nailing down the recursive structure of language, then, is an essential step towards a full explanation of our language mastery, and of lots more besides, as we shall see (to wit: of our grasp of concepts, of our conception of ourselves as fully rational agents, of the nature and character of norms and of the very possibility of rule-governed behaviour).

#### 1.4. Context and Semantic Content

The explanation is both neat and reassuring. The question now is whether it is true, that is, *empirically* adequate. So far, it looks little more than an *a priori* stipulation justified on transcendental grounds—only compositional languages are *learnable*, we are told.

There is a familiar (and subversive) fly in the ointment though, namely, the ever-present effects of context on semantic content, the evident divergence between the bare surface structure of our words and the richness of content they seem able to *express*, and not just convey, in different contexts.<sup>19</sup>

The semanticist standard reply is that ordinary language sentences are to be taken as (variously implemented) *shorthand* for eternal sentences that do express complete, fully disambiguated thoughts. Recovering meanings-in-context is thus a question of completing the missing bits, a job done by craftily (albeit mostly unconsciously) executed recourse to contextual clues.

For any (apparently) incomplete (i.e. semantically underdetermined) sentence, then, there is a corresponding complete thought and the gap between the two gets filled via appropriate contextual additions to the surface form of the (abbreviated) sentence.

To a first approximation, the semanticist response to the challenge from context can be seen as subdividing into three broad strategies:<sup>20</sup> the Eternalist strategy (where NL sentences are shorthand for fully-disambiguated sentences in some *other* language, somewhat immune to the vernacular's defectiveness); the Indexicalist strategy (where content determination is carried out either at the metalinguistic level via the assignment of values to various indices or at the Logical Form level, where hidden parameters awaiting contextual setting reside); and the Retreatist strategy, where the scope of semantics is restricted to the processing of (possibly sub-propositional, possibly merely dis-

<sup>19</sup>Note that in this dissertation I am almost exclusively concerned with *semantic*, not communicated content.

<sup>20</sup>More details in ch. 3.

quotational) minimally conceived content.

I will end up endorsing a radically minimalist version of the last strategy. My task in the chapters to follow, then, will largely be aimed at undermining the other two positions (and most varieties of the Retreatist strategy too).

Before giving the broad plan for my dissertation, let me point out two more puzzles.

All semanticist views (and some pragmaticist views as well) assume that we can somehow discipline the transition not just from atoms to complexes, but from signs to thoughts as well.

Signs, it would seem, can point in indefinitely many directions (that's what they *do*, that's what they *are*), and any system of signs (no matter how rigorously formulated) will inherit that essential instability. Indeed, it is in virtue of their expressive and adaptive *power* that signs are able to be bearers of content.

But concepts, and thoughts more generally, seem to be individuated *as the thoughts they are* in virtue of their *univocity*: there must be no *doubt* as to *which* thought I am entertaining for the thought to be the thought it is.

And so we face the problem of providing a mapping from *constitutively ambiguous entities* (our signs) to *constitutively unambiguous ones* (our thoughts).

This puzzle, I think, adds yet more pressure on the idea that there *is* a solution to the Extension Problem, and that it goes via appeal to compositionality, for the flight from context seems to rely on the very possibility that determinacy of content *could* be secured for our signs (if we can't determine *which* thought-in-context an occurrence of an ambiguous sign points to, it seems unclear that we have succeeded in doing semantics).<sup>21</sup>

The final large puzzle I will consider centres on the issue of the normativity of meaning. This is in fact where all the considerations above converge. For it seems as if the normative constraints that supposedly attach to our use of expressions flow both from claims about conceptual structure and about the (highly privileged) status of the deliverances of the compositional machinery.

Given certain beliefs about the atoms (presumably reflecting certain beliefs about the structure of our concepts), the corresponding

<sup>21</sup>Recall that truth-conditional semantics is supposed to give an account of how complex meanings *functionally depend* on constituent meanings. Without an answer to concerns about *global* indeterminacy, it remains however unclear whether the compositional machinery is operating in the manner *assumed* by the semanticist; it is unclear, that is, whether the argument-value pairings have been properly primed.



beliefs concerning the complex meanings thereby derived will be *rationally mandated*. That's what grounds the authority of language over us, that's what guarantees the objectivity of meaning, or so it seems.

But if so, the question arises as to *why* we should abide by the verdicts of the compositional machinery: if facts about competence derive from *biological* constraints, as the linguist insists, it is unclear that those constraints could ever acquire *normative* weight for us; if they are grounded in facts about extension (as the semanticist-philosopher seems to assume, given her insistence on a robustly construed notion of reference),<sup>22</sup> then it is equally unclear how they could be *implemented* ahead of use—it is at the very least implausible that in fixing the atoms we have thereby *definitively* fixed the extension of *all* expressions containing them.

These are all rather large questions, of course, but it seems to me that before we can seriously engage in semantics we have to address them, at least so as to become clear on *what* we are doing when we formulate meaning-theories for a fragment of the vernacular.

In that respect, I think it is fair to say that broadly speaking this study is a reflection on *what meanings could not be*. It is for another day to say something more precise about what they *actually* are. And towards that goal, I'll be in fact perfectly satisfied if, to borrow from a rather wonderful passage in Russell (1940: 10), the reader will judge that in these pages I've managed no more than to substitute “articulate hesitation for inarticulate certainty” over these matters.

One further point: I heard it reported that Timothy Williamson has said in conversation that the good thing about formal models is that they keep us honest. If there is a lesson to be drawn from the pages ahead is that this is the case only if those models are themselves honest—if they are, that is, accurate representations of what they purport to model. In some cases, honesty demands that we abstain from modelling for a little while yet.<sup>23</sup> Or rather: that we are clear as to *what* we are modelling.

The question before us then is not why the theory of meaning is so hard, but rather what a theory of meaning should be a theory *of*. Specifically, what features of competence are we modelling when we insist that the only empirically adequate theory is a *structure-reflecting* one?<sup>24</sup> And what normative force do those features *really* have for us?

In the pages ahead I try to provide the beginning of an answer to

<sup>22</sup>See e.g. Peacocke (2008).

<sup>23</sup>What we must beware, that is, is the *appearance of exactitude* that models all too readily provide (Wittgenstein 1931-34/1974: II.I.7).

<sup>24</sup>Wright (2001c: 45-6).

questions such as these.

### **1.5. The Plan**

Here, then, is the plan for the chapters to follow.

In chapter 2 I state a number of theses which I take to be the defining ones for semanticism. I show that they form a class of jointly incompatible commitments. I choose nonsense as a problem case for compositionality and I argue that it forces the semanticist to abandon either the learnability or the compositionality constraint. The escape route I adopt, going radically minimalist about content, is incompatible with another key semanticist thesis, namely, that grasp of meaning is grasp of truth-conditions (robustly conceived).

In chapter 3, I consider the account of atomic meanings given by both the semanticist and the pragmatist and I conclude that on neither account does interpretation come out as a process of rational choice between candidate bearers of content. Again, I suggest the lesson from indeterminacy is that we ought to embrace an ineradicably minimal conception of content.

In chapter 4 I turn my attention to the meaning of the logical constants and I argue that indeterminacy worries extend to the very heart of the compositional machinery.

Chapter 5 examines the view that logic is the science of reasoning. Unsurprisingly, I conclude that a defence of this claim requires endorsing content-minimalism.

In chapter 6 I conclude my dissertation by sketching a radical view of content minimalism and I try to show how it can solve the puzzles I had been considering over the course of the previous chapters.



## Chapter 2

# What Compositionality Could Not Be

### 2.1. Introduction

In mathematics, you don't understand things.  
You just get used to them.

*John von Neumann*

Disputes in metasemantics do run deep. So much so that there is little prospect of a minimal theory of meaning that could isolate the (platitudinous) common ground among participants—the intersection of opinion in the theory of meaning may well be empty, that is.<sup>1</sup>

For all that, here's a prime candidate for the role of Platitude-in-Chief in the area:

**Creativity Considerations (CC):** Speakers possess an unbounded ability to understand novel sentences effortlessly<sup>2</sup>

<sup>1</sup>The idea of a minimal theory for a given subject matter seems to have originated with Greenough (2003: 237). See also Johnston (1988: 38), Wright (1992: 34) and Smith (1994: §2.8).

<sup>2</sup>See e.g. Husserl (1900-01/2001: 70), *Tractatus* §4.024 and 4.03, Frege (1980: 79) and (1914: 210, 225), Chomsky (1955-56/1975: 131), (1965: 6, 161-2) and (1966: 59), Davidson (1967: 17) and (2004: 8), Pustejovsky (1995: 1), Szabó (2000: 80), McGilvray (2001: 6), Lepore and Ludwig (2005: 7), Pietroski (2005a: 13-14), Moro (2008: 40), Hornstein (2009: 1). Other labels for (nearly enough) the same phenomenon are 'productivity' and 'systematicity' (something that Evans' (1982: §4.3) *Generality Constraint* and Davies' (1981: 53-55) *Mirror Constraint* had aimed to capture); see Johnson (2004) for discussion. For Fodor (1997) and Kamp and Reyle (1993: 7), systematicity is what explains linguistic creativity. As Evans (1981a: 327) notes, what requires theoretical explanation is our ability to track *any* extension (however minimal) of the semantics (the standardly drawn finite/infinite contrast is bogus). Fricker (1982: 65) is refreshingly explicit that CC provides "the justifying basis" for attributions of a rich semantic structure to NL (*that* claim is my target here). She also makes a tidy connection between the Extension Problem and CC—it is speakers' ability to understand sentences in an expanded language that shows the original language to be (appropriately) structured.

CC is not just taken to be *centrally* platitudinous—a brute piece of data from which we must somehow start in our theory-building efforts. It is, rather, deemed to be both *the* phenomenon to be *explained* and (hence) the key *motivation* for the positing of certain constraints on the *structure* of our theory, most notably that it be *compositional*.

CC is, in short, the crucial touchstone against which the empirical adequacy of one's semantics has got to be assessed.

My purpose in this chapter is to challenge its alleged platitudinous status and to question the claimed explanatory connection between CC and the principle of compositionality.<sup>3</sup>

I shall argue that compositionality and CC are in fact incompatible. My weapon of choice is nonsense—I'll say why in a moment. Before I can fully unravel the plot, however, I need to do a bit of preparatory work.

## 2.2. Creativity and Compositionality

CC is fairly obviously a claim about the epistemology of meaning. For competent practitioners, coming to understand complex meanings (even previously unencountered ones) is an *immediate*, effortless affair. We unreflectively *know* what a novel sentence means; *how* is *that* possible—it is then asked.<sup>4</sup>

While it leaves the precise shape of that epistemology rather unclear, CC assigns a central role to the notion of understanding within a theory of language (the phenomenon crying for explanation is that what is *unbound* is our *understanding* of indefinitely many novel meanings), and something needs to be said about that notion right at the outset.

So here's another thesis, which is far from uncontroversial (at this point, we're already leaving the domain of platitudes in this area), but still something of a majority view:

**Epistemic Conception of Understanding (EPU):** To understand an expression *e* is to know the meaning of *e*.<sup>5</sup>

<sup>3</sup>The literature is fairly unanimous in its defence of (the centrality of) CC. I can think of two exceptions: the abrasive, book-length attack on the very idea of CC contained in Baker and Hacker (1984) and the rather more restrained Margalit (1978).

<sup>4</sup>The Kantian overtones of this sort of question (dutifully noted in Baker and Hacker 1984: 346) typically prompt a transcendental reply. I'm sliding between 'understanding' (some sort of state/ability) and 'coming to understand' (some sort of process) here. The slide is fairly common in the literature—again a lone exception is Baker and Hacker (1980/2005: ch. 17, §3). I'm not sure how harmful the slide might be.

<sup>5</sup>Read this as a conditional:  $(U(S) \rightarrow K(\mu(S)))$ . As Williamson (2000: 110, fn. 4) notes, the

EPU makes the notion of understanding more precise and by so doing, it gives us a reading of CC whereby the alleged truism becomes the slightly more controversial (but still *prima facie* plausible) claim that what requires theoretical explanation is *how we come to know* the meaning of novel sentences.<sup>6</sup>

There is another way of bringing out the puzzle that CC highlights, and it doesn't involve appeal to speakers' understanding. So here's another definition:

**Extension Problem (EP):** The main technical problem a semantic theory faces is how to extend a given semantics for a certain class of expressions in a language to a semantics for either a) a suitably related class of expressions in the same language or b) a class of expressions in a (conservatively) expanded language.<sup>7</sup>

Patently, EP involves no appeal (not even covertly) to understanding or indeed to speakers.<sup>8</sup> On the face of it, there is no epistemic reading available for EP, and yet one feels that what CC is about is pretty much continuous with (one form of) the technical problem that EP brings to the fore.

Both statements, that is, seem to involve an important *structural fact* about language: assignments of semantic values are not (and *must* not be) *random*, or stipulated on a case-by-case basis,<sup>9</sup> and it is precisely their *systematicity* that allows us to achieve competence

converse does not have to hold. The label probably originates with Pettit (2002a) who argues at length against EPU. Dummett is committed to EPU and anticipates Pettit's objection in his (1981: 308). For a classic statement of EPU see Platts (1979/1997: ch. II).

<sup>6</sup>One example of this gloss is Tennant (1987: 10). Alternatively, one may appeal to belief rather than knowledge, as Pettit (2002a) does—see also Gross (2005) and Pettit (2005). For other conceptions of linguistic understanding see Peacocke (2008), the ability-based accounts in Baker and Hacker (1980/2005: ch. 17), Devitt (1997: 268-9), Johnston (1988: 38-9) and the quasi-perceptual ones in Ziff (1972: 3), Fricker (1982: 56) and (2003), McDowell (1987: 99) and (1997: 114-17).

<sup>7</sup>The term comes from Hodges (1998: 16) and Westerståhl (2004); see also Hodges (2005: 42-3). Katz and Fodor (1963: §2) call it the *Projection Problem*. The problem is to give a (systematic) solution that *respects* (in roughly the same way in which the term is used in formal logic, e.g. Hinman 2005: 17) the meaning—i.e. the categorial status—of the lexical items *and* of the connectives (informally: the semantics must appropriately deliver the intuitive truth-conditions for sentences). See Westerståhl (1998: 638-9) and Lasnik and Uriagereka (2005: §1.2) for discussion. Chomsky (1957: 14-5) argued that a grammar ought to *mirror* a speaker's competence in *understanding* "an indefinite number of new sentences"—see also Chomsky (1965: 4-5). This is as clear a statement of the connection between CC and EP as you can hope to find.

<sup>8</sup>One may of course wonder *what would be the point* of insisting that we solve the EP if it were not for the need to make the semantics *computationally* viable for beings such as ourselves. So, appearances to the contrary, even EP has an epistemic flavour after all.

<sup>9</sup>Frege (1980: 79) motivated his insistence on PoC by pointing out that the principle removes the need to stipulate case-by-case conventions for each complex meaning.

in the language.<sup>10</sup>

Solve EP *tidily*, one wants to say, and you go some way towards giving an answer to CC too—it is *because* of the *disciplined* connections holding between the meanings of the atoms and those of complex expressions that we are able competently to move between the two classes of expressions, the suggestion goes.

Indeed, here are two oft-given replies to the questions posed by CC and EP:

**Principle of Compositionality (PoC):** The meaning of a complex expression is solely a function of the meanings of its parts and their syntactic mode of combination.<sup>11</sup>

and

**Generative Principle (GP):** There is an unbound recursive syntactic operation (Merge) that given *any* two already formed strings of lower complexity generates a new expression by merging them into one syntactic unit.<sup>12</sup>

It seems obvious that PoC provides a fairly precise answer to one form of the EP, namely, that of extending a semantics for the atoms and the compositional operations to a semantics for all sentences in the language.

And while it is not immediately clear how to get an answer to CC from GP (we'll deal with this a little later), there's a near-immediate connection between PoC and CC—so immediate, in fact, that few bother to make it explicit.

For it is a straightforward matter to state an explicitly epistemic (and fairly plausible) formulation of PoC:

**Epistemic Principle of Compositionality (EPoC):** A speaker *s* knows the meaning of a complex expression just in case *s* knows the meanings of its parts and of their syntactic mode of combination.<sup>13</sup>

<sup>10</sup>Even the iconoclastic Davidson (1986: 436) insists on this.

<sup>11</sup>That compositionality provides the most natural explanation of linguistic productivity is argued for even by critics of the post-Fregean project in semantics such as Barwise and Perry (1983: 127).

<sup>12</sup>For just one example see Chomsky (1995b: 226).

<sup>13</sup>Note this is in effect a *closure* principle: (tacit) knowledge of meaning is closed under the compositional operations. For discussion of the notion of tacit knowledge see Davies (1981: 74, 84), Evans (1981a: 336-7) and Wright (1986c). It seems fairly uncontentious that Davidsonians are committed to EPoC. Notoriously, Davidson's claim was that knowledge of an appropriate theory of meaning would suffice for (compositionally-based) understanding of a language. See Lepore

With EPU in place, it is then even more straightforward to derive a version of PoC that directly answers the question posed by CC:

**Principle of the Compositionality of Understanding (PoCU):**  
 a speaker *s* understands the meaning of a complex expression  
 just in case *s* understands the meanings of its parts and of their  
 syntactic mode of combination.<sup>14</sup>

PoCU gives a precise answer to the puzzle stated in CC: we understand novel meanings because of our antecedent familiarity with their parts and their mode of composition.<sup>15</sup> If we remove its epistemic layers and work our way back to PoC we find a precise answer to EP too, as we just saw above.

This also gives us a handle on a more general answer to CC. There are structural facts about *language* (language has a certain syntactico-semantic structure whereby semantics is a function of syntax) which PoC identifies, and there are (parallel) structural facts about *speakers' competence* such that by (tacitly) knowing the structural facts about language (more weakly: by understanding them), speakers are able to secure immediate understanding of infinitely many complex meanings. The way they do this is neatly modelled by the equation  $EPU + EPoC = PoCU$ .

If so, we are justified in attributing *just as much structure* to meaning as *suffices* to model speakers' competence in interpreting novel

and Ludwig (2005: 23-7) for a formal statement of the Davidsonian position. Searle (1994: 644) explicitly speaks of PoC as a principle concerning understanding.

<sup>14</sup>This is the philosopher's view of compositionality. See for instance Quine (1966: 75-6) (who considered the principle necessary—no less!—and tied it directly to CC), Tennant (1987: 31), Horwich (1990/1998: 35) and (1998: 155), Devitt (1997: 282), Heim and Kratzer (1998: 2), Collins (2003: 403) and Borg (2004: 21). By contrast, Fodor and Lepore (2001: 45) argue that understanding need not be compositional. Davidson (1967: 21) seems to deny EPoC, but all that needs adding is his holism. Dummett is the author most clearly committed to EPU and PoCU and to the related contention that a theory of meaning must be a theory of understanding, see e.g. his (1973: 92), (1976: 288), (1981: 308). For Wright (1976: 224) the distinctive feature of a theory of meaning is precisely that it purports to illuminate “the epistemology of the transition from understanding of subsentential components of a new sentence to recognition of the sense of the whole”. Chomsky (1966: 121, fn. 9) quotes approvingly from Juan Huarte's 1575 *Examen de Ingenios* where it is said that “Understanding is a generative faculty”.

<sup>15</sup>Conditional on the notion of the understanding of the atoms being made clear, of course. Typically, this is something semanticists will refuse to do—see e.g. Lepore and Ludwig (2007: 4). Tennant (1987: 31) claims that we derive the meaning of the atoms from their embedment into complex expressions and *then* re-combine them into further, possibly new complexes. There is a characteristic tension here between the *fact* that meanings are learnt in context (under embedment) and the CC-*claim* that our understanding of the complex is parasitic (and posterior to!) our understanding of its components. See Janssen (2001) for a discussion of this issue in relation to Frege's *Context Principle*.



sentences (the slogan: structure is a model of competence).<sup>16</sup> Let's call the theorist who advocates a picture of this kind a *semanticist*.<sup>17</sup>

In fact, this answer to the CC puzzle establishes an intriguing (albeit entirely to be expected) connection with Tarski's work on truth. Recall that for him the defining mark of a formal language  $\mathcal{L}$  was that the sense of every  $\mathcal{L}$ -expression was uniquely determined by its form.<sup>18</sup>

We can then say that, on the picture of language proposed by the semanticist, grasp of the sense of a NL expression is achieved (solely) via grasp of its *form*. As long as we understand the form of an expression, we are in a position to understand its meaning.

Call this the *Understanding-as-Grasp-of-Structure* (UaGS) conception.<sup>19</sup>

PoCU and UaGS thus neatly encapsulate the semanticist's optimism about the prospects for accurately capturing our competence within a formal network.

We also have a reciprocal (and just as neat) relation holding between CC and PoC. Compositionality gives *one* (intuitively correct) answer to the CC puzzle (the existence claim); CC, it is then argued, is *only* (plausibly) explained by PoC (the required uniqueness claim).<sup>20</sup>

The upshot is: NL semantics *must* be compositional on fully *a priori* grounds—and this is another claim that I'm challenging in this chapter.

Two more things to note. I've just said that it has been routinely

<sup>16</sup>A thesis defended in e.g. Davidson (1967: 25) (and Lepore and Ludwig 2007: 19), Fricker (1982: 49, 65) and Sainsbury (2005: 49).

<sup>17</sup>A semanticist will typically be committed to more; for instance, she will insist that semantic content is fully determined by the structural facts discussed in the text (Stanley 2007), and that linguistic competence is (very nearly) entirely the province of the language faculty (Borg 2004). Chomsky's position is more subtle (and highly elusive: see e.g. Chomsky (1975b: ch. 2) and the rejection of PoC in his 1975a) and has changed substantially over the years. Chomsky (1965: 136) was staunchly semanticist—semantic interpretation depends *only* on lexical items and compositional principles (but see the qualifications in fn. 9 therein). Already Chomsky (1970a: 67–8) and (1972a: 134) were more guarded. Chomsky (2007) is altogether more cautious on this issue.

<sup>18</sup>Tarski (1935: 166). I read 'uniquely' as including 'solely' too: nothing but the form of an expression determines its sense.

<sup>19</sup>*Locus classicus*: Russell (1914/1993: 53). See also Davidson (1967: 26), Evans (1976: 57) and (1981a: 324–25), Davies (1981: 80), Dummett (1981: 310), Wright (1981: 44), Fricker (1982: 50, 52), Hinzen (2006: ix). To motivate UaGS, Davies (2000) (and 2003: 40) invokes two problems, that of *meaning despite use* (sentences which have a different meaning to the one speakers standardly employ) and that of *meaning without use* (the intuition that unused sentences nevertheless do possess a determinate meaning which we somehow *uncover*) and claims that the only solution is positing tacit knowledge of a structurally-rich system of rules—and so does Fricker (1982: 59).

<sup>20</sup>That CC motivates a commitment to PoC is stated in e.g. Tennant (1987: 37), Grandy (1990), Dever (2006: 633), Borg (2007: 355, fn. 36) and Cohnitz (2005: 54–55).

argued that 1) *only* PoC (or GP) could explain CC. The claim is thus a *transcendental* one regarding the very possibility of language—non-compositional languages (and knowledge thereof) would not be expandable (with the obvious *modus tollens* back to PoC in the vicinity).<sup>21</sup>

In response to objections from pragmaticists, it has also been routinely argued that 2) we should keep distinct the *formal* constraints on a semantics (PoC) or on a syntax (GP), which are intended to address the EP, from epistemic principles such as PoCU and EPU which instead apply to a different branch of semantics (psychological semantics, or some such label).<sup>22</sup>

My first point in this chapter is that these two claims cannot coherently be defended together. PoC *only* explains CC if it has some *demonstrable* connection with the epistemic (note: *not* the psychological) aspects of meaning and meaning computation.<sup>23</sup>

Patently, EPoC and PoCU make no claim as to the *psychological* reality of the (explanatory) recursive mechanisms they posit. Those mechanisms are merely *models* of competence, a competence however that is essentially epistemic in character—linguistic competence, it is claimed by the Chomskyan and the Fregean alike, is *knowledge* (or *cognisance*)<sup>24</sup> of rules (and recursive ones at that).

In turn, CC sustains the compositional constraint on a semantics only if we can establish the appropriate connection between syntactic structure and the epistemic properties of expressions in virtue of which we acquire competence in their use, properties that themselves arise *in virtue of that very structure*.<sup>25</sup>

In other words, *a priori* reasoning can only impose PoC on a semantics via its connections to PoCU—no other (epistemically depleted) version of PoC can fully explain CC (given its essentially epistemic character) and be justified by it.<sup>26</sup> I'm sticking my neck out on this

<sup>21</sup>See e.g. Ziff (1960: 60), Davidson (1967: 17). I entirely share Wedgwood's (2005: 22-7) doubts as to this move. A weaker form of the argument would be to say that PoC is the *best explanation* of CC, as (Lasnik 2009) suggests.

<sup>22</sup>Partee (1988), Borg (2004), Soames (1984, 1985, 1989), Janssen (1997: 446), Hodges (1998: 24).

<sup>23</sup>A point duly noted in Bonnay (2005: 43).

<sup>24</sup>Chomsky (1986: 265ff.).

<sup>25</sup>The only way for a semanticist (e.g. Westerståhl 1998: 641) to reject claims that PoC is an entirely trivial principle (e.g. Zadrozny 1994) requires precisely an appeal to the fact that the undesirable, trivially compositional meaning functions bear no "correspondence" to intuitively acceptable (i.e. *epistemically* plausible) ones.

<sup>26</sup>Even an *a posteriori* justification of the claim that NL has a compositional semantics would have to appeal to facts to do with interpretation, i.e. with ways in which we actually compute

one, so it deserves yet another label:

**Epistemic Constraint on Explanation (ECE):** Only PoCU can provide an *a priori* explanation of CC and be motivated by it.<sup>27</sup>

And semanticism without ECE is blind—jettisoning this constraint is no way out of the difficulties I will raise for the semanticist.

We need one more principle on the table:

**Meaning Determination Principle (MDP):** the meaning of an expression is entirely determined (controlled) by the syntactic structure of the expression.<sup>28</sup>

We thus have another equation: GP + MDP = PoC. Bracket that aside for a while yet—its role should be clear anyway, given what I’ve been saying above (the equation will be needed to make sense of the Chomskyan linguist appeal to CC to motivate claims about the role of GP).

We are now almost ready to state the main line of this chapter—just a little more work, and we’ll be there.

### 2.2.1. Towards Nonsense

Let’s now recall the standard semanticist claim that semantic theories are taken to be stating the connections in virtue of which certain (systematic) relations among assignments of semantic values to sen-

meanings. Lasersohn (2009) has argued otherwise (unconvincingly). Kempson *et al.* (2001: §1.1), Stokhof (2002) and Cann *et al.* (2005: §1.3) argue for the opposite. Chomsky (2000: 12) himself has defined e.g. the *displacement property* in terms of interpretation; and so a fact about how we interpret sentences (i.e. we treat expressions *as if* they were in a different position from the one occupied at surface structure) becomes a fact about competence (as noted in Cook and Newson 2007: 33) and *hence* about linguistic structure itself. Incidentally, as Evans (1976: 51) pointed out, the only theory-neutral way to define semantically complex expressions is in terms of speakers’ *understanding* (non-atomic expressions are those that *require* grasp of structure to be understood).

<sup>27</sup>‘Justified’ might be a more apt term here. Hodges (1998: 10), Szabó (2000: ch. 3) and Fodor and Lepore (2001: 45) reject ECE. I find their claim (and arguments) puzzling.

<sup>28</sup>MDP is the key dogma of semanticism, from Carnap (1942: §7), Chomsky (1965: 75, 136, 162), (1966: 93) and (1972a: 131-134, 178), all the way up to Fodor and Lepore (1991: 333), Cappelen and Lepore (2005a: 2) and Stanley (2007). For Janssen (1997: 427) and Stanley (2000: 34) MDP is entailed by PoC (recall the ‘solely’ qualifier in its formulation). Collins (2007) reminds us that modern thinking in syntactic theory lends little support to MDP. Syntax both *under-* and *over-*determines content (there isn’t enough *structure* in the syntax to pin down semantically expressed propositional content but there is also redundancy created by the *Copy* operation). For all that, Chomskyans routinely claim that Universal Grammar is a system for pairing sounds/signs and meanings—see e.g. Chomsky (1970b: 12,14) and (1981: 17), Hinzen (2006: 154, fn. 4), (2006: 111), (2009: 16, 19), Webelhuth (1995: 3), Bošković and Lasnik (2007: 1), Boeckx (2008b: 2).

tences (and expressions more generally) hold (e.g. entailment, synonymy and so forth).<sup>29</sup>

Relatedly, they are also expected to be making predictions as to the truth-conditions of sentences (or the propositions associated with them) and, crucially, as to which meanings will be *impossible* (whatever *that* means).<sup>30</sup>

In other words, semantic theories have got to encase what Katz and Fodor (1963: 183) called *projection rules* from the lexicon to the set of complex expressions.<sup>31</sup>

It is those rules that discipline the semantic behaviour of expressions under embedment. It is those rules that impose normative constraints on usage (to be a *rational* language user is to abide by those rules). It is (tacit) grasp (or internal representation) of those rules that constitutes our linguistic competence (our competence has *exactly* the same structure as our language). Indeed, it is those rules that make our vernacular *learnable*.

And if the semanticist picture of the language machinery is correct, it is those rules that correctly predict the emergence of nonsense when words are combined in certain (presumably illicit) ways.<sup>32</sup>

The semanticist's commitment to some version of PoC controlling the projection rules and to a PoCU-explanation of CC will therefore entail a corresponding commitment to the preservation by the compositional operations of a certain range of properties.<sup>33</sup>

Above all, meaningfulness and understanding must be preserved

<sup>29</sup>This view of semantics stems from (at least) Katz and Fodor (1963) and is fairly widely endorsed, e.g. Katz (1981: 207), Soames (1985: 159), Larson and Segal (1995: 3), Szabó (2000: 51). For a recent contrary voice, see Horwich (2010: ch. 8).

<sup>30</sup>See Husserl (1900-01/2001: IV, §10), Katz and Fodor (1963: 175), Chomsky (1965: 3, 157), Routley (1966: 178), Davidson (1967: 21) and (1984: xiii), Katz (1981: 207), Pustejovsky (1995: 40-41), Webelhuth (1995: 4), Barker and Jacobson (2007: 2-3), Hendriks *et al.* (2010: 21).

<sup>31</sup>See also Chomsky (1957: 15) and (1965: 154). I'm also targeting the more specific *Projection Principle* in e.g. Chomsky (1981: 29), (1982: 4-9) and (1986: 82) that the  $\theta$ -marking properties of lexical items (i.e. the properties that *type* the thematic roles of lexemes) are "represented categorially at each syntactic level". In fact the motivation for the principle in his (1981: 31) is virtually the same acquisition-argument given in support of PoC. See also Culicover (1997: 99). The Projection Principle is now no more (see Chomsky 1981: 187-9 and 1995a: 390), its role taken over by the *Merge* operation (Chomsky 1995a: 396-8). Geeraerts (2010: §3.2) has a good discussion of the history of projection rules within the generative enterprise.

<sup>32</sup>As we shall see, the trouble for the semanticist comes from the *need* to discipline the interaction between lexicon and compositional rules in such a way that nonsense can be predicted in advance (assuming semanticism, the EP for NL has *no* solution, I'll claim).

<sup>33</sup>As Williamson (2003b: 264, fn. 17) points out, some properties, e.g. pragmatic ones such as presupposition, won't generally be preserved.

under composition.<sup>34</sup> And here's where trouble awaits the semanticist, I contend. In order to carry out the explanatory task required by the CC truism, the semanticist needs to show that meaningfulness and understanding are preserved (or provide a *systematic* explanation of *why and how* they fail to be so preserved) and, more importantly, that their preservation is ultimately dependent on the (formal) compositional constraints on the semantics (and thus that the *correct* rules—presumably some version of the traditional Tarskian ones extended to deal with some version of possible worlds semantics—are those that preserve the relevant properties).<sup>35</sup>

Specifically, I shall argue that nonsense poses a problem for this view, because nonsensical sentences are such that the *meaningfulness* of the components (and our knowledge/understanding of what that meaningfulness consists in) is not preserved by the compositional operations.

Nonsense, that is, is a failure of *closure*: the meaningfulness of the components does not add up to a meaningful whole (i.e. even *minimal* increases in syntactic complexity may engender loss of understanding).

Conversely, EPoCU implausibly attributes *semantic omniscience* to speakers (knowledge of the atoms is taken to guarantee knowledge of *any* combination thereof).<sup>36</sup>

In short, *contra* the obvious reading of CC, the set of meaningful sentences is *not* closed under the (unrestricted) compositional operations.<sup>37</sup>

My main contention, then, is that this puts the semanticist in a dilemma—the first of four that I raise for her in this chapter—since either she has to radically disconnect epistemic versions of PoC from the version adopted in her theory of meaning (thus severely weakening the force of CC as the main motivation for *insisting* on the

<sup>34</sup> Obviously, it is an interesting question which *level* of meaningfulness (and of understanding) needs to be preserved for the truth of CC (and of semanticism). See fn. 157, p. 50, for discussion.

<sup>35</sup> The idea of closure/property preservation is fairly clear in Fodor and Lepore (2002: 1), where PoC is glossed as the idea that complex expressions *inherit* the syntactico-semantic properties of their constituents. Clearly, closure is another way of saying that the rules *do* genuinely project.

<sup>36</sup> We could also say that NL is not compositionally *conservative* (adding novel complex meanings changes the meaning of the atoms). For just one example of the semanticist commitment to the preservation of meaningfulness/understanding under composition see e.g. Fricker (1982: 64). Note that the problem for EPoCU is highly general and not restricted to nonsense. Replies along the lines of Dowty (2007: 27) or Everaert (2010: 83) would be wide of the mark.

<sup>37</sup> *Contra*, in particular, Katz and Fodor (1963: 171). The failure of closure may not worry those, like Chomsky, who do not deem the set to be i) recursively enumerable anyway; ii) of much relevance to the formal enterprise. More later.

compositional constraint) or she is obliged to increase the amount of information contained in the lexicon so that illicit combinations can be correctly predicted (and disqualified) ahead of use—PoC, that is, must also explain the *contrapositive* of CC (why speakers do *not* understand nonsense).<sup>38</sup>

The required increase to *every* lexeme's information content will however be such as to make the lexicon itself *unlearnable*.<sup>39</sup>

**Semanticist Dilemma (SD) I:** To define the characteristic function for the set of meaningful expressions, the semanticist will either have to 1) inflate the lexicon and compositional rules beyond learnability so as to regulate *all* possible meaning-combinations in advance of use or 2) abandon PoCU, and thus lose the main motivation for insisting that a semantics be compositional.<sup>40</sup>

In an attempt to save compositionality as the only explanation of our linguistic creativity *and* of the learnability of our languages, the semanticist is forced to inflate the lexicon (and/or the compositional rules) to the point where the learnability constraint is violated.<sup>41</sup>

The semanticist view thus contains a fatal tension between learnability and creativity, and the commitment to PoC makes it impossible to satisfy both requirements. Accordingly, the allegedly truistic connection between CC and PoC turns out to be an illusion, or so I shall argue.<sup>42</sup>

This should suffice by way of stage-setting.

In the next section I examine the phenomenon of nonsense. In section 4 and 5 I move on to examine possible exit points for the semanticist. I conclude that the supposedly platitudinous status of CC is

<sup>38</sup>For a recent discussion of the inexhaustible variety of contextual sense-modulation and the difficulties it creates for the lexicon see Wilson and Carston (2007: §2).

<sup>39</sup>As Uriagereka (2008: xvii) notes, it is often assumed that questions of memory limitations only affect performance. This is not so; they do significantly affect competence too (as Uriagereka eloquently shows).

<sup>40</sup>(1) seems obvious (or will become so during the course of the chapter); the point about (2) is that the semanticist may choose to go epistemicist and insist that the function delivers a verdict in all cases but that (like e.g. the Ackermann function) it may in general outrun *our* ability to compute its values. If so, CC must be given a different explanation.

<sup>41</sup>On the learnability requirement, see e.g. the classic statement in Davidson (1964: 3, 8). The requirement persists, see e.g. Stanley (2000: 34), who thinks that admitting violations to PoC due to extra-linguistic context would make language unlearnable.

<sup>42</sup>If my line of reasoning is sustained, we then face the further questions of a) explaining away the plausibility of the semanticist's insistence on the compositionality constraint; b) sketching out a role for systematicity and grasp of structure within a non-compositional framework, both tasks for another day, I fear.

unwarranted and that CC, in and of itself, does not provide support for the insistence that NL semantics respect PoC.

Before all that, there is one more issue I need to deal with.

### 2.2.2. Why Nonsense?

One might question the choice of nonsense.<sup>43</sup> Why should we (and the semanticist in particular) worry about nonsense? Aren't nonsensical sentences (NS) peripheral, if not outright exotic cases anyway?<sup>44</sup>

After all, CC can be taken to require that we explain how we understand novel sentences *when we do understand them*—it wasn't meant to cover *all* sentences at all.<sup>45</sup> Can't we therefore simply rule out NS as outlandish cases of no import to the standard processing of well-behaved meanings? Couldn't we for instance just stipulate that the meaning function is not defined for NS? I'll deal with this last strategy in the next section. Let me address the first point right away instead.

I contend that nonsense cases are absolutely central to compositionality, that they are in fact the litmus test for the principle. Here's why.

First, with nonsense there is no interference from the haze of use—there is no noise from distracting use-familiarity to hide the workings of the machinery, that is. Moreover, and for the same reason, NS are *genuinely* novel cases, and thus precisely the ones at the heart of CC, the ones we are under theoretical obligation to explain.

In contrast, the classic textbook examples that are supposed to provide intuitive support for PoC are at most *unusual*, rather than starkly novel, in that they combine familiar words in familiar patterns: the oddity is purely in the startling juxtaposition of perfectly familiar kernels that give only a mild rattle to our conceptual scheme—the novelty is subdued, the trick these examples pull but a cheap one.<sup>46</sup>

Not so with *echt* cases of nonsense, cases where meanings do clang together, where categories clash and derivations crash—the failure of

<sup>43</sup>Patrick Greenough has urged me to address this issue.

<sup>44</sup>This remark is indeed often made, but it is nonetheless curious. As Dummett (1974: 22) points out, it is precisely disputed cases of applicability that make perspicuous the precise specifications of the rules that supposedly determine the meaning of an expression (that's why *Zettel* §440 is so unsettling, for the very possibility that we could *always* construct awkward cases obviously threatens the general decidability of our predicates).

<sup>45</sup>To claim that we understand indefinitely many sentences leaves it open that there may be indefinitely many we don't understand. I'm being charitable here. Although this option is logically possible, standard statements of CC are clearly not meant to leave this particular loophole open.

<sup>46</sup>See e.g. Chierchia and McConnell-Ginet's (1990/2000: 6) "I saw a pink whale in the parking lot" and the similar examples in Lycan (2000: 3) and Dever (2006: 633-4).

understanding in such cases is truly catastrophic, not just an oddity to be dismissed away as purely peripheral.

The fact is that it is in the face of authentic novelty, and *only* then, that we can hear the cogs in the compositional machinery being laid bare as they grind to an embarrassing halt (or at least it *seems* as if they do).<sup>47</sup>

Suddenly, in the face of nonsense, the normally non-overtly inferential mechanisms that (supposedly) *deliver* meanings to us are *demanding* additional input from speakers, for they appear impotent to construct a meaning for the troublesome sentence *unless supplemented by additional input from the speaker/hearer*.<sup>48</sup>

Ultimately, the idea challenged by nonsense is that there are *canonical* grounds for acceptance for sentences and that those grounds are presented to us on a plate by the compositional machinery (that's what talk of the objectivity of meaning boils down to; that's, allegedly, the source of whatever normativity meaning has for us).

It seems instead obvious that NS do not have canonical grounds attached to them by the compositional machinery; and if so, I can see no reason why we should think that the machinery *does* succeed in attaching canonical grounds to run-of-the-mill cases either.

And this is indeed why NS also challenge one other key commitment behind truth-conditional semantics, namely its *representational stance*, its distinctive claim that to know the meaning of a sentence is to know what the world would have to be like for the sentence to be true.<sup>49</sup>

On the semanticist picture, a sentence provides us with a purported representation of reality that we may then compare to the world; our assertoric practices with that particular sentence are thus a function of the particular sentence-world connection determined by its syntactico-semantic structure.

But for us to be able to judge whether or not a sentence is assertible in a context it is necessary that we can make sense of the purported claim made by sentence *before* we look out to the world.<sup>50</sup>

Nonsense puts pressure on this idea, as we shall see, for we don't even know what a NS *says*, and if so, we lack the means for com-

<sup>47</sup> Am I conceding that there *is* a compositional machinery? Perhaps. But even imaginary machines can make imaginary noises.

<sup>48</sup> Chomsky (1965: 76, 149). Similarly, Horwich (1998: 155) states that, given an understanding of the constituents, *no further work* is needed to gain understanding of the complex.

<sup>49</sup> See e.g. Heim and Kratzer (1998: 1). Throughout, my use of 'representationalist' and its cognates is to be taken in Taylor's (1980) and Brandom's (2000) sense. A concise critique of representationalism is in Read (2010: 558).

<sup>50</sup> As Wittgenstein (1914-16/1998: 23) noted.



paring sentence and world—the most basic test, it would seem, for establishing *whether or not* one is *competent* in a language.

We see here another facet of UaGS: on that view, grasping the meaning of a complex expression is grasping a *definite representational claim* about some portion of reality, which is *then* used for truth-value assessment. And the idea is that the structure of that claim is compositionally determined (and *solely* so).<sup>51</sup>

Nonsense challenges this idea, for it is conceded on all hands that NS require *further* input from speakers (*that* is indeed *the* mark of nonsense)<sup>52</sup>—left on their own, as it were, NS provide at most *sub-skeletal* components for truth-value assessment.

There is therefore another, closely related target that needs mentioning, and it's the idea, still current in much contemporary semantics, that we give the meaning of a sentence by stating its truth conditions (the fact that those conditions are worldly is what makes semanticism a representationalist enterprise).

Given this (more or less straightforward) equation of meaning and truth-conditions, EPU entails our last definition in this chapter:

UaKTC: To understand a sentence is to know its truth-conditions.<sup>53</sup>

I shall argue that nonsense puts pressure on the claim that understanding is grasp (indeed, *knowledge*) of truth conditions (note the connection with UaGS: grasping the *form* of an expression is grasping its *truth-conditional* contribution under embedment).

Indeed, a commitment to UaKTC gives rise to the second dilemma for the semanticist, which I shall discuss at greater length later in the chapter:

SD II: Given UaKTC, the semanticist will have to say either that we understand nonsense or that we do not. In the first case, she will have to weaken the classic conception of truth-conditions, in the second case she will have to reject CC. Neither horn is

<sup>51</sup>On many views, the representational aspect of language is its central semantic fact (Soames 1989: 182). See Chomsky (2000: 132) and (2003: 292) for a contrary view. My own view is fairly close to Chomsky's (minus his baffling insistence on nativism): the representational structures provided by language are radically minimal (but that's because mental content is radically minimal too). Representationalism needs not be committed to what Ludlow (2003: 145) and (2011: 134) called the *Language/World Isomorphism*. A much weaker assumption will do just as well.

<sup>52</sup>Chomsky (1965: §4.1).

<sup>53</sup>Again, read this as a conditional ( $U(S) \rightarrow KTC(S)$ )—see fn. 5. A more general version would have it that to understand an expression is to know its truth-conditional contribution under embedment. See Carnap (1942: §7) for a classic statement of UaKTC.

compatible with full-blown semanticism.

And if all this is not enough, nonsense matters for yet another, absolutely fundamental reason.

Suppose we think that it is unreasonable to demand that a semantics give an account of the grasp/possession of satisfaction conditions for predicates and atomic sentences more generally. The task of a compositional semantics, we have been told time and time again (perhaps at least since Stalnaker (1980: 905), perhaps since long before that), is to give an account of the functional relations holding between expressions under embedment.<sup>54</sup>

Nonsense, I shall argue, puts pressure on that more modest claim too. In fact, I think it shows that claim to be either an utterly trivial one or outright empty.

Lastly, nonsense puts pressure on the idea that a theory of meaning could be *modest* (in the sense made familiar and defended by McDowell),<sup>55</sup> that it could, that is, be piggy-backed on an antecedent understanding of the meta-language. For with nonsense there is no help forthcoming from the meta-language: the semanticist cannot invoke a prior understanding of the metalanguage as is routinely done in cases of ambiguity, indeterminacy, vagueness or our mastery of the logical connectives.<sup>56</sup>

With nonsense, that is, understanding, if absent from the object language, also breaks down across the hierarchy of languages. And once again this lays bare the actual scope of the recursive mechanisms posited by the semanticist.

My conclusion will therefore be that the meaning stipulations for the lexicon and the grammar do not reach out as far as the semanticist claims; they are largely *provisional* on further intervention by speakers—a fact obscured, with “normal” sentences, by the haze of use, which hides the true extent of the contribution by speakers’ judgments to the determination of complex meanings.

I trust this suffices to motivate my choice of nonsense as the means

<sup>54</sup>And those relations *must* have epistemic import: Lepore and Ludwig (2005: 139) state that “the aim of a compositional meaning theory [is] to put us in a position to understand any sentence [...] on the basis of understanding its elements and their arrangement”—as clear a proof of the semanticist commitment to EPoCU as you can wish to get.

<sup>55</sup>McDowell (1987, 1997). The term was introduced in Dummett (1974: 5).

<sup>56</sup>McDowell would resist this argument, for the whole point about his notion of modesty was that robust theories of meaning presuppose a mythical standpoint situated outside our practices—see his (1981a: 342). I agree but again I draw different conclusions from this (perfectly sound) remark. There is a further reason why becoming clear about nonsense is crucial to our theories. As Russell (1940: 216) noted, if we take sentences, rather than propositions, to be the truth-bearers, then before we can assert e.g. the Law of Excluded Middle we need to know which sentences are significant.

by which to raise difficulties for some of the defining theses of semanticism. It is now time to get my argument going in greater detail.

### 2.3. Varieties of Nonsense

So far, we have been working with a relatively informal notion of PoC. Interestingly, *formal* statements of PoC nearly always make a fundamental assumption, namely, that the expressions involved are *already* meaningful (for complex expressions, the domain of the meaning function is restricted to the class, or algebra, of those grammatical constituent terms that are *already* mapped to some elements in the class, or algebra, of meanings).<sup>57</sup> Indeed, another way of looking at PoC is to say that it ensures that the class of grammatical (well-formed) statements is *exactly* the class of meaningful expressions.<sup>58</sup>

Arguably, this assumption reflects the fact that analytic philosophers tend to view language through (first-order) logic-tinted glasses, as it were, with the completeness theorem forcing on them a commitment to an analogous and equally neat correspondence between grammaticalness and meaningfulness—the (unofficial) slogan: *grammars do not leak*.<sup>59</sup>

There is a rather awkward problem with this requirement, however, for, as a matter of fact, grammaticalness and meaningfulness may (and do) come apart in NL.<sup>60</sup> Moreover, NL lacks well-defined criteria for well-formedness, grammaticalness and meaningfulness anyway.<sup>61</sup>

Consequently, this puts pressure on the semanticist's commitment to *forward* (or *bottom-up*) compositionality,<sup>62</sup> that is, on the idea that

<sup>57</sup>Montague (1970c: 225-7), Dowty *et al.* (1981: 42-43), Janssen (1997: §8), Hodges (1998: 10) and (2001: §5), Westerståhl (1998: 636), Westerståhl (2002: 248), Pagin (2005: 307), Dever (2006: 635).

<sup>58</sup>Emmon Bach's (1976) gloss on PoC as a rule-to-rule hypothesis in effect conjectures that to every syntactic rule there corresponds a semantic rule that delivers a meaning for *any* expression.

<sup>59</sup>*Contra* Edward Sapir's famous dictum that *all* grammars leak (in more ways than one). Russell (1940: 209) was clear that NL leaks but also argued that we should (and could) construct a language where all and only legitimately constructed sentences do have a meaning, just like Frege (1893/1998: I. §32) had insisted—Russell (1923: 65) however candidly admits this is a chimera. As far as formal languages are concerned, unique readability and the truth-definition (rather than completeness) guarantee that every well-formed expression is assigned a meaning.

<sup>60</sup>Chomsky (1957: 15).

<sup>61</sup>Wilks (1971, 1975), Chomsky (1972a: 121), (1980: 127-8) and (2003: 288). As Chomsky (2009: 15) notes, his (1955-56/1975: ch. 4) had raised difficulties for a commitment to well-formedness that were not given due prominence in his (1957).

<sup>62</sup>The term 'forward compositionality' comes from Patterson (2005: 328).

constituent meanings determine the meaning of embedding complexes *in full generality*, and hence that, given EPoC, an understanding of the atoms suffices for an understanding of an unbound (and fully unrestricted) range of complex meanings.

Furthermore, the vagueness in the notion of well-formedness also has a serious impact on the referential/representational commitments of the semanticist, as I shall argue a couple of sub-sections down the line.

Before worrying about *that*, though, let me stress that the gap between grammaticalness and well-formedness already puts pressure on the *linguist's* attempts to discipline *syntactic* behaviour so as to mark out as licit those derivations that do not result in nonsense.

So, prior to turning my attention to the semanticist's claims regarding the *semantic* reach of expressions, in the next sub-section I shall first take issue with the Chomskyan *syntactocentric* view of nonsense. Much of what I say here will apply to the semanticist as well and it is helpful, I think, to separate my attack in this way.

### 2.3.1. Degrees of Nonsense

Now, the absence of a precise criterion to demarcate well-formed sentences from defective ones does not of course mean that we do not have a robust handle on well-formedness issues regarding unquestionably un-/acceptable sentences.<sup>63</sup>

And so at a first approximation we can endorse Chomsky's suggestion that the class of sentences be divided into

**Completely ungrammatical sentences:** Pure nonsense, as it were, strings that are not recognisable as belonging to any language or that are clearly ungrammatical by the standards of a recognised language: e.g. 'ab sur ah', (Wittgenstein 1932-35/2001: 64) or 'brought admires Tom' (Chomsky 1955-56/1975: 130-1).

**Partially grammatical sentences:** The ones of most interest to us, that is, cases where we have "perfectly grammatical strings that are incongruous on nonsyntactic grounds" (Chomsky 1965: 158): paradigm case, the *Chomsky-sentence* 'colorless green ideas sleep furiously'.<sup>64</sup>

<sup>63</sup>Searle (1983b: 78) and (1994: 638) argued against Derrida that we can draw a distinction even if we lack the means of drawing it rigorously and precisely. The point is sound, if intriguing, but it is certainly inimical to the Fregean project (Frege 1906: 303) and to those mathematically-minded theorists committed to an ideal of *underlying* exactitude for our meanings, see for instance Carnap (1936: 424). See Glendinning (1998: ch. 5) for a critique of this ideal.

<sup>64</sup>Chomsky (1955-56/1975: 145), (1957: 15). One might detect a tension between a diagnosis

**Fully grammatical sentences:** “Normal”, non-aberrant sentences, posing no difficulty to syntactico-semantic processing.<sup>65</sup>

So, the proposal, from Chomsky and others, is that we rank defective sentences along a *degree of grammaticalness* scale in terms of their departure from uncontroversial cases of grammaticality (or equivalently, the amount of “repair” needed to make the offending sentences acceptable).<sup>66</sup>

We thus have a new version of the requirement that linguistic theory generate sound-meaning relations: the requirement is now taken to be fully general, that is, *all* expressions, whatever their status, will be assigned a pairing.<sup>67</sup>

In addition, what the theory also needs to do is *explain* (by appropriate modelling) speakers’ ability immediately to rank sentences along the grammaticalness scale.<sup>68</sup>

More specifically, and as Chomsky (1955-56/1975: 227) had insisted, it is of crucial importance that the theory be able to provide a clear demarcation between all-out deviancy and *semantic* deviancy.<sup>69</sup>

In less charged vocabulary, we want an explanation of what distinguishes *grammatical* from *ungrammatical nonsense*.<sup>70</sup>

Note that we are thus *adding* to CC: what we now have to elucidate

of deviancy formulated, at least occasionally, in nonsyntactic terms and Chomsky’s implementation of a purely syntactic remedy (note also the two senses of ‘grammatical’ involved in Chomsky’s taxonomy and in his own description of the Chomsky-sentence). As the next few footnotes make clear, Chomsky is well aware of the fluidity of the notions involved. Aarts (2007: 46-52) has a useful discussion of Chomsky’s position on these matters.

<sup>65</sup>The taxonomy is proposed in Chomsky (1955-56/1975: 131). See also Chomsky (1965: 76-7). Note that Chomsky (1957: 23) flirts very briefly and uncharacteristically with the idea that for fully grammatical sentences we can state the conditions under which they are true.

<sup>66</sup>See Cruse (1986: §1.2): syntactically-challenged sentences, as it were, can be patched up by sorting out constituents that belong to the *closed set* of expressions (those that are fully grammaticalised, i.e. relatively immutable over time and in function) while semantic defectiveness will typically require substitution of open set items (i.e. items from the lexicon). A similar distinction is invoked in Brinton and Traugott (2005: 1ff.). The trouble for this proposal is that, as Cruse himself recognises (20, fn. 4), the distinction between open and closed set elements is not an absolute one. This analysis of deviancy simply pushes the problem one step deeper.

<sup>67</sup>See e.g. Chomsky (2003: 288) and (2009: 15). And the question, on the semanticist side, will be: does understanding get preserved across all such pairings?

<sup>68</sup>Chomsky (1955-56/1975: 132, 142). Note also that establishing the correct combinatorial rules would give us a unique insight into “the nature and source” of speakers’ productivity (p. 135). Once again, nonsense is not an exotic component in the theory of meaning, but one of its central features.

<sup>69</sup>As usual, semi-medical terminology is invoked to castigate departures from “normality” as in various ways *defective*: the unruly sentences are ‘deviant’, ‘aberrant’ and ‘anomalous’, see Chomsky (1965: 76-7), Larson and Segal (1995: 46-7), Buekens (2005) and Jackendoff (1990: 53).

<sup>70</sup>Chomsky (1955-56/1975: 146).

is not just the unbound *productivity* of the language faculty but also *our* capacity to discern (and account for) *differences* in its output.

### 2.3.2. (De-)selecting Nonsense

At first blush, Chomsky's suggestion with regard to nonsense is entirely aligned with the philosophical tradition on the matter: once we have established the (possibly a bit leaky) trichotomy above, we need *selectional rules* that restrict derivations and classify output accordingly.<sup>71</sup>

The basic idea is that the Chomsky sentence, like the familiar old chestnuts (e.g. Russell's "Quadruplicity drinks procrastination" and Carnap's "Caesar is a prime number"), violates categorial restrictions—it offends, that is, against the logical syntax of language.<sup>72</sup> Therefore, the theorist's task is to devise projection rules that explain and predict the emergence of nonsense.

Very roughly, here is the general shape of this strategy.<sup>73</sup>

Lexical entries are assigned (in fact: they *are*) a set of *syntactic* features (e.g. for nouns, these include Common, Count, Animate, Abstract).<sup>74</sup> In effect, we are *typing* expressions by specifying features that dictate which contexts the expressions can occur in and introduce a further constraint on complex strings: they have to be not just well-formed, but *well-typed* too.

Broad category violations will produce ungrammatical nonsense (e.g. in 'brought admired Tom' a verb occupies a position normally assigned to a nominal). Violations at the *sub*-categorical level (particularly with regard to the thematic roles assignments attached to verbs) will in general produce grammatical nonsense (e.g. the Chomsky sentence, where several such restrictions are broken).<sup>75</sup>

<sup>71</sup>Actually, Chomsky considers two possible strategies: restrict syntactic derivations by selectional rules, or delegate the marking of deviance to the semantic module. Roughly, it is the distinction between direct and derivative generation of the deviant sentences. See (Chomsky 1955-56/1975: 131) and (1965: 227).

<sup>72</sup>Russell (1940: 209, 213), Carnap (1932: 67). The diagnosis of a clash of categories is venerable (recall Aristotle's 'justice is not a square number' in *Magna Moralia*, I, 1182<sup>a</sup>14). See the comprehensive résumé in Horn (1989/2001: §2.3). Jackendoff (1990: 54) spells out the idea nicely from a generativist perspective. A more recent discussion is in Magidor (2009).

<sup>73</sup>For the details, see Chomsky (1955-56/1975: ch. 4) and (1965: ch. 2, 4.1).

<sup>74</sup>More precisely, they are sets of syntactic, phonological and semantic features—Chomsky (1965: 214). The selectional features, however, are generally taken to be syntactic. Note that, obviously enough, we are relying on a *fixed* understanding of what counts as abstract and animate here. One might well wonder how the *syntax* would be able to do *that* independently of a given conceptual scheme that will have strong *semantic* (and indeed *ontological*) features.

<sup>75</sup>For Chomsky (1965: 87), the lexicon is the repository of idiosyncratic properties (this includes

The explanation is neat, and the syntactic machinery gives a fairly solid implementation of the intuitive reaction to nonsense, e.g. being a prime number is not the kind of thing that can happen to the conqueror of Gaul, whilst having a particularly bad day in mid-March is.

The strategy also seems to provide a nice response to the worries about the CC-PoC connection. With the appropriate selectional restrictions in place, there is no reason to worry unduly over nonsense. We cannot understand NS simply because the illicit combinations of meanings arising therein *already* offend against *syntax*, let alone semantics.

Our incomprehension is actually *evidence* (as good as it gets, in fact) that we *are* competent: we, and the compositional machinery, can spot nonsense a mile ahead—there’s no fooling *us*.

### 2.3.3. The Autonomy of Syntax

Note that Chomsky is insistent throughout that selectional rules are operating on *syntactic* features—that’s why it is a question of *competence* (and not of performance) that we be able to rank grammaticality and meaningfulness *precisely as we do*. This is, in fact, one way in which the much-discussed autonomy of syntax thesis gets to do some interesting work (the selectional features work *independently* of semantic constraints: they are *already* primed at the syntactic level).<sup>76</sup>

Recall also that for Chomsky (1955-56/1975: 227) the ability to classify sentences on the grammaticality/meaningfulness scale is one of the key *manifestations* of our competence: that’s why it doesn’t much matter that NL doesn’t have a clear-cut criterion for well-formedness in general.

All’s not well, however.<sup>77</sup> Consider the rather extraordinary—to properties relevant to semantic interpretation!). Moreover (p. 88), the lexicon carries the burden of providing information that *determines* “the degree and manner of deviation” of strings not directly generated.

<sup>76</sup>See for instance the claim in Chomsky (1955-56/1975: 87) that “semantic notions are of *no* assistance in the determination of formal structure” (my emphasis). Characteristically, elsewhere (e.g. 1965: 77) Chomsky is less intransigent. Later still, (1975b: 54-5), he defends the autonomy of syntax view as the idea that “the language faculty constructs an abstract formal skeleton invested with meaning by the interpretive rules”. I question the claim that there is a *strict* connection between interpretive rules and the syntactico-semantic structure posited by the formalist. Specifically, interpretive rules (and I doubt the use of the term ‘rule’ is appropriate here) impinge on syntax in deeper ways than Chomsky concedes (or conceded before ca. 2006). Hinzen (2006: 154, fn. 4) is still adamant that the task of a Chomskyan grammar is to derive “expressions-under-an-interpretation” in a manner that fully respects MDP.

<sup>77</sup>The usual tack here is to provide an interpretation that makes (some) sense of the C-sentence.

my mind at least—claim in Chomsky (1955-56/1975: 146) that the Chomsky sentence (C-sentence) is grammatical (if defective) by virtue of being an instance of the same sentence-form as the “perfectly normal”:<sup>78</sup>

1) Revolutionary new ideas appear infrequently

In fact, Chomsky calls the C-sentence “an absurd semi-English sentence” (presumably because ideas cannot be green, and sleeping furiously is something no-one can engage in).

But what of (1) itself? ‘Revolution’ is a term introduced in the 14th century to denote a property of the motion of celestial bodies. It was first applied to political upheavals in the mid-fifteenth century.<sup>79</sup> Only much later did it apply to abstract terms such as ideas.<sup>80</sup> And the same, *mutatis mutandis*, is true of ‘appear’—abstract notions are by definition not part of the phenomenal realm.<sup>81</sup>

That (1) counts, in Chomsky’s I-language, as ‘normal’, then, seems hardly a matter of syntax, especially if we think of it in terms of *universal* grammar (presumably, broad verb subcategorization belongs

But Chomsky (1965: 76, 149) had anticipated this move and would simply retort that the distinction he’s after is still in place: deviant sentences *demand* rather than impose an interpretation. My strategy is different: I attack the idea that “normal” sentences have, in general, an interpretation imposed on them by the machinery *in virtue of deep facts about our mind* (or anything else, in fact). For I’d reverse Foucault’s (1969: 101-02) judgement regarding the C-sentence and argue that without a context “normal” sentences cannot be considered meaningful either. See also Searle (1980), Ross (1981: 55-6) and Glendinning (2007: 86).

<sup>78</sup>There is a curious tension between this claim and Chomsky’s (1966: 65) attack on exactly the same view as held by Bloomfield. In fn. 22 on p. 124, Chomsky also defends MDP with respect to “quite novel” sentences.

<sup>79</sup>And of course as Chesterton wryly noted, the application to politics had a deeper meaning than intended: strictly speaking, it amounts to a *plus ça change* verdict, rather than recognition of a genuine change of trajectory.

<sup>80</sup>According to the OED, one of the earliest such uses is due to Burke in 1796 but the ‘revolutionary ideas’ pairing only appears with G.B. Shaw in 1919. Tracing earliest occurrences of a particular usage is a dangerous game to play, but all I need is the undoubted gap between the astronomical use and its (mis-)application to abstract ideas (and similarly for ‘appear’). See Hacking (1983: 8-9) for some other early examples of the “abstract” use of ‘revolutionary’.

<sup>81</sup>The point generalises to other cases discussed in Chomsky (1965: 149) such as ‘Sincerity admires John’ and ‘Golf plays John’. Both sentences can *easily* be *given* not just interpretations (Chomsky countenanced that circumstance) but can also be taken to be part of ‘normal’ (self-interpreting, as it were) English given enough currency within a specific conceptual scheme: the Medieval anthropomorphisation of virtues made Sincerity a perfectly suitable entity to figure as the subject of a verb such as ‘admire’; an admiring “new-age” coach may well confide to a friend that John is not just very good at golf, he’s become so good that golf plays John and not viceversa. These cases seem to me perfectly good, perfectly normal and liable to *immediate* interpretations by speakers who have become familiar with their changed *grammatical* status.



to the principles and not to the parameters-setting function of grammar).<sup>82</sup>

In all likelihood, the Chomskyan will reply that the setting of subcategories is a question of the “proper description for a *fixed* linguistic corpus”,<sup>83</sup> where a measure of idealisation is invoked in the corpus-fixing move: once the subcategorisation parameters are (re-)set, the machine will churn along happily.<sup>84</sup>

But this is not good enough. CC poses a puzzle about our *unbound* understanding. It now turns out that our understanding is actually *constrained* not by syntax but rather by the *conceptual scheme* which happens to be in force at a given point in time (and is exemplified in a given linguistic corpus)<sup>85</sup>—e.g. there was a time (between the 14th and the 17th century) when the now “normal” (i.e. norms-compliant) (1) would too have counted as absurd semi-English (so much for the presumed *inviolable* status of selectional rules, then).<sup>86</sup>

And it is the *speakers’* (and the machinery’s) forgetfulness of (1)’s rather murky past history that *now* enables it to have gained respectability as a fully normal, no longer deviant, member of the class of grammatical sentences—curiously enough, it is that very same forgetfulness that supposedly *mandates* the ‘normal’ interpretation for (1) rather than sending us off on a quest for an interpretation (as was the case with the C-sentence in 1955).

If my critique of Chomsky’s view of nonsense is on the right track, it therefore seems as if our competence in detecting degrees of grammaticalness would more properly be described as an exercise of *imaginative rationality*, to adapt Lakoff and Johnson’s (1980: 193) apt label

<sup>82</sup>I.e., *that* verbs *are* subcategorised may well be a universal feature of language and that a particular subcategorisation will establish a necessary connection between theta-role assignments and the suitability of lexical items to figure as arguments in the derivation is also taken to be a universal feature of languages. See e.g. (Lasnik and Uriagereka 2005: 6)—note though how their fn. 5 on p. 29 first concedes that ergative languages may represent an exception, only to explain it away in the next breath because of the claimed availability of an analysis in transformational terms. For the Principles & Parameters phase in Chomsky’s thought, see his (1995b: ch. 1) and Culicover (1997).

<sup>83</sup>Chomsky (1955-56/1975: 147), my emphasis.

<sup>84</sup>Perhaps something like this lies behind Chomsky’s (1965: 75-9, 111, 153) discussion of the possible division of labour between the syntactic and the semantic module with respect to the implementation of selectional restrictions on derivations. Or perhaps the Chomskyan would be content with the claim that our theory is modelling our intuitive grasp of the *order of deviance* of sentences (Chomsky 1965: 152).

<sup>85</sup>I decided to concentrate on (1) as a target, but similar considerations would apply to our ever-evolving understanding of scientific terms and even to natural kinds terms, as is familiar from discussions of these issues by e.g. Austin (1946: 88) and Putnam (1962: 239). Indeed Searle (1980: 230-1) made the very same point with respect to the hallowed ‘Snow is white’.

<sup>86</sup>For that strong claim, see Chomsky (1965: 150).

for our ability to see structural similarities holding across different domains, rather than of *syntax-led* competence—we do not blindly *follow* the verdicts of the machine: *we* set its parameters and *judge* which categorial restrictions *ought* to be in place, given the state of our knowledge.<sup>87</sup>

Indeed, even if the Chomskyan were to retreat to a view whereby syntax merely *implements* time-indexed conceptual constraints, we would still be a long way away from having established the *virtually necessary* connection between the question posed by CC and the answer given by GP (or, as we shall see in a moment, PoC), for facts about competence were supposed to show deep facts about our *mind*, not about the history-bound vagaries of our conceptual schemes.<sup>88</sup>

Now, Jackendoff (1990: 155) has contended that behind the conception of selectional rules in Chomsky (1965) there is the assumption that a neat correspondence exists between conceptual structure and syntax.<sup>89</sup>

The point is, however, that *that is precisely what a linguistic theory ought to establish*, and not just take for granted, or delegate to some other branch of enquiry.<sup>90</sup> After all, it has been claimed time and time again that linguistics is an *empirical* discipline accountable to the facts; that it is best construed as *bio-linguistics*, and that its proper object of enquiry is the internal representations of the competent speaker—language is a *natural* object on this view.<sup>91</sup>

If so, we should be making claims about its *actual* features, and my main contention throughout this dissertation is precisely that *the facts*

<sup>87</sup>Note that in some way a view of this kind is perfectly in keeping with one aspect of Chomsky's conception of linguistic creativity, namely, *our* ability to adapt words to context. What I'm questioning is whether Chomsky has accurately located the source of that ability (i.e. whether he has succeeded in identifying *what kind of rules* genuinely govern our language mastery: and *this*, as stated in e.g. Chomsky (1972a: 141) is indeed "the fundamental problem" of linguistic theory).

<sup>88</sup>See Chomsky (1995a: 385), Boeckx (2006: 10), Hinzen (2007: 27), Hornstein (2009).

<sup>89</sup>Chomsky (1965: 77) openly, but noncommittally, considers this assumption. It seems clear that his official position is strongly opposed to such a view, however, e.g. (2003: 292). Elsewhere, however, Chomsky has appeared to defend a nativist view of conceptual structure with regard to lexical items, e.g. Chomsky (2000: 62). I think this is one more example of his characteristic oscillation between apparently incompatible positions. In any case, it is absurd to think that our concepts could *everywhere* be constrained by the language faculty with respect to features such as "intending, causation, goal of action, event, and so on", for *these* notions are themselves subject to revision.

<sup>90</sup>Chomsky (1965: 75) draws a distinction between questions of presentations (how to state a theory) and justification (how to justify a given choice of presentation). My point is: the distinction is a good one, but without an answer to the justification question, one's claim that the privileged form of presentation properly models (let alone represents) our competence may well lose much of its cogency.

<sup>91</sup>See e.g. Chomsky (2000: ch. 2) and (2009: 13, 16).

*about our competence* mandate a view of content as radically minimal (even more minimal than Chomsky's already deflationary conception) and that *even the sub-categorization principles that we attribute to syntax are provisional* and do not necessarily point to *fixed* structures in the mind and certainly not, as the semanticist (but not Chomsky) would have it, to facts about reality.<sup>92</sup>

In short, I shall claim that syntax is indeed autonomous, and in much bolder ways than even Chomsky countenanced: *all* (grammatically well-formed) sentences, precisely as *Tractatus* §5.4733 had warned, are legitimately constructed. Accordingly, and *contra* Chomsky, my main conclusion in this section is that grammatical nonsense is a nonsensical notion.

I will return later to the Chomskyan framework, and from a much more sympathetic perspective.<sup>93</sup> For now, mention of a claimed correspondence between conceptual structure and syntax gives me a good transition point for moving the spotlight back on the semanticist.

## 2.4. Categories and the Laws of Meaning

Let's briefly take stock. We started from the claim that CC is the key fact to be explained and that PoC is the only (or the best) explanation available. Our problem was that nonsense seems to threaten that claim, for by definition we *cannot make sense* of nonsense (and so *contra* CC, understanding is not unbound). To address that, we needed some way to screen off nonsense, but when we tested Chomsky's syntactocentric version of UaGS, we found it wanting: the relevant sub-categorization features do not appear to be sufficiently independent of context-relative (perhaps even interest-relative) conceptual structures.

Suppose now that we set aside the claim that *syntax alone* can discipline nonsense.<sup>94</sup> We could, that is, counter the argument of the last sub-section somewhere along these lines. My critique of Chomsky focused on the C-sentence and its formerly-roguish-and-now-reformed counterpart (1). But in both cases the category clash was not as severe as, say, in the Carnap sentence 'Caesar is a prime number'—a patch was *all too easily* available (and indeed successfully im-

<sup>92</sup>See e.g. Chomsky (2000: 126-7).

<sup>93</sup>If anything, the lesson from this section is that Chomsky's view of grammatical nonsense is in tension with his views regarding reference and representationalism.

<sup>94</sup>I'm taking into account the fact that the notion of syntax at stake here is wider than the philosopher's standard view of the matter, as e.g. Chomsky (2003: 287) makes clear.

plemented and then forgotten about) in the case of (1).<sup>95</sup>

However, no amount of shifting in our conceptual scheme will *ever* alter the status of the Carnap sentence, since if we tried that we'd be banging our head against the wall of *conceptual* necessity. Therefore, the objector might conclude, grammatical nonsense *does* exist and *some* selectional rules *are* cast in (conceptual-ontological) stone.<sup>96</sup> Moreover, we can know those rules *a priori* and consequently predict *terminal* nonsense ahead of embedment and in complete innocence of the current state of our conceptual scheme.

This, or something like this, is in effect Husserl's position. It deserves consideration.

#### 2.4.1. Husserlian Meanings

In discussing the C-sentence, I ended up suggesting that meaning stipulations are provisional, and that selectional rules are (by and large) indexed to a conceptual scheme.<sup>97</sup> The objection we are considering would also argue that this gets things back to front, that my suggestion would weaken any claims we might make as to the objectivity of meaning—and if meanings were *really* up for grabs in the manner I seem to suggest, *what* could possibly explain our agreement in judgements and the day-to-day (ostensibly unimpeded) communicability of content?

Surely, the objection goes, at best all that I have established is that our *epistemic* access to facts about meaning is fraught with difficulties. We do get things wrong, no doubt about that. But conceptual analysis is precisely the task of getting clearer about the ultimate (and immutable) nature of concepts and our semantics parallels that (ever-evolving) process.<sup>98</sup> It doesn't follow from my discussion of (1)

<sup>95</sup> And, if rather lively cases of synesthesia are contemplated, this is so even for green ideas. As for the 'colorless green' clash (on the face of it, a conceptual impossibility), reflect that 'revolutionary new' is equally 'impossible' ('properly speaking', 'revolution' means a kind of motion that doubles back on itself: 'revolutionary new' would thus be a contradiction in terms by the formalist's own lights, one would have thought).

<sup>96</sup> I actually think the C-sentence was meant to capture *fundamental* selectional rules, but that's by-the-by.

<sup>97</sup> Jackendoff (1997: 31) reads Chomsky as diagnosing the deviancy of the C-sentence in terms of conceptual structure violations. I can find no evidence to support that claim, apart from the "wondering aloud" kind of remarks that I have copiously cited. The main point Chomsky intended to derive from the example, as I stressed repeatedly, was that the relative difference of the sets of grammatical and meaningful sentences is non-empty and that "any search for a semantically based definition of 'grammaticalness' will be futile" (1957: 15). Equally importantly, he used that sentence to attack probabilistic explanations of competence (1955-56/1975: 145).

<sup>98</sup> That is Frege's view, as usefully discussed in the already cited Burge (2005: 55ff.).

that meanings are *everywhere* provisional, that is. All that follows is that it might take us a while to pin down fully complete meaning-specifications.

I'll defend my view against objections of this kind in greater detail in the chapters ahead. For now, I want to give this objection as good a chance as I can, although I should also note in passing that I think that the Carnap sentence can be dealt with in the same manner as the C-sentence.<sup>99</sup>

I grant that on one intuitively attractive reading PoC is *the* principle that can best enforce the *contractual* view of meaning, which in turn exemplifies in a very natural way the idea that meaning is (and must be) objective.<sup>100</sup>

PoC, that is, plays a *dual theoretical role*, since it provides not just a neat answer to EP and CC, but it also does *normative* duty, in that it stipulates that a commitment to the atomic meanings *binds* speakers to accepting *unconditionally* the complex ones delivered by the compositional machinery (the grounds for one's beliefs about constituent meanings are *eo ipso* grounds for specific, and *rationally mandated*, beliefs about the complex meanings).<sup>101</sup>

Meaning derivations, on this picture, do not determine genuinely *new* concepts. They are not, then, *instruments of conceptual change*.<sup>102</sup> Rather, they merely *unveil*, by means of word-recombination, previously constituted ones.

From this perspective, no sentence is genuinely *novel*—all possible sentential meanings are *already* contained in the atoms: you couldn't understand the atoms without *already* understanding the sentences in which they get embedded.<sup>103</sup>

The crucial claim for our purposes, then, is that *the range of all possible combinations is already written into the atoms ahead of use*.<sup>104</sup>

<sup>99</sup>There is in any case a further point to note here, namely, that CC makes a *universal* claim about the *absolutely* unbounded character of our linguistic understanding. My argument against Chomsky's conception of nonsense in the previous section is all that was needed to challenge that claim. What the current objection threatens is my further claim that the class of grammatical nonsense is empty. I think we can *always* expand the argument in the text to deal with the Carnap sentence and its cognates in the same way I dealt with the Chomsky sentence, but I won't argue for that here.

<sup>100</sup>Wright (1980: 19), McDowell (1984: 221).

<sup>101</sup>What I am questioning by my appeal to nonsense is above all the thought that PoC *can* enforce norms of rationality of this kind in relation to linguistic competence.

<sup>102</sup>Wright's (1980: 5) useful gloss on the Wittgensteinian conception of proof. See Dummett's (1959: 177) for a contrary view.

<sup>103</sup>This, again, shows why any non-epistemic version of PoC just won't make the needed explanatory work with respect to the CC puzzle.

<sup>104</sup>See e.g. Evans (1976: 325). Note the analogy—once again a legacy of the logic-led view of

It is this that gives PoC its bite—no other principle could succeed in combining explanatory adequacy *and* normative import so neatly.<sup>105</sup>

In short, and to paraphrase Wittgenstein: the semanticist's (unspoken) motto is that compositionality ensures that *there are no surprises in semantics*.<sup>106</sup>

The objection under scrutiny now faces a crucial question. To secure the objectivity of meaning in this manner, and in accordance with MDP, the lexicon must incorporate *all* possible future use in its specifications. If so, we have to ask: *How* can we prime the (sub-)categorical status of the atoms ahead of their embedment in a range of (linguistic and non-linguistic) contexts that is, *ex hypothesi*, unbound (and whose cardinality is in fact uncountable)?<sup>107</sup>

More importantly still, what *justification* can we offer for a given choice of nonsense-busting categorial restrictions?<sup>108</sup>

NL—between this view of the role of PoC and the idea that the class of consequences of a given set of axioms is already contained in the generating base. However, strictly speaking, the theorems cannot properly be said to be contained in the axioms alone, but rather in the axioms *and* the structures that satisfy them. Properly unpacked, that is, the claim is a (ternarily) *relational* one. And in the case of NL the tricky question is of course: what are the structures the lexical base is true of?

<sup>105</sup>And as anticipated: it is precisely the plausibility (and “naturalness”) of this claim to fulfil a double role that gives it so much grief.

<sup>106</sup>*Tractatus* §6.1251. The role of PoC in guaranteeing objectivity of meaning goes hand in hand (rather uncomfortably) with a *purely contingent* fact about learnability: case-by-case convention-making, we are told by Frege and today's semanticists, would make language unlearnable. There is claimed to be an *a priori* link between our specific contingent cognitive capacities and the structure of the languages accessible to us. However, *a)* there exist perfectly coherent non-compositional, connectivist-based accounts of language structure and non-rule-based learning patterns, and *b)* there is ample (although by no means incontrovertible) empirical evidence that meaning computation is *in fact* often non-compositional and that we *do* negotiate complex meanings pretty much on a case-by-case basis, especially in the case of compound noun phrases, see Dąbrowska (2004: ch. 2 §3.1), Costello and Keane (2005) and Dunbar (2005). On compound nouns, see also Patterson (2005) and Partee's (1984a: 165) classic attempt to save PoC by going idiomatic. For his part, Chomsky (1972a: 169, fn. 40) argues that compound noun phrases require *ad hoc* semantic rules. Given their ubiquity, this seems hardly conducive to the generality claims invariably made about the compositional machinery—Dunbar (2005) convincingly shows that the selection of the relevant constituent semantic features essentially relies on speakers' *decisions*.

<sup>107</sup>Chomsky (1965: 161) himself had noted the “crucial but [...] relatively unexplored” question of the need for the semantic component to characterise “field properties” (semantic relations holding *across* lexical entries, such as antonymy) that cannot be given in the individual lexical entries and yet robustly discipline sentence-formation rules. He was, that is, acutely aware of the essentially holistic character of lexical properties. One might object to the claim that the class of relevant contexts is uncountable. The class of *linguistic* contexts is certainly (denumerably) countable. Well, the fact is we are dealing with *semantic* acceptability and, as we shall see, *scenarios* play a crucial part in acceptability judgements. Assuming (at least) a first-order formalisation for NL, there are uncountably many interpretations available.

<sup>108</sup>Something like this point is made in Russell (1940: 228); see also Chomsky (1972a: 133).

On the syntactocentric view we just examined, the derivational constraints were the result of the *syntax-led* interaction between the various lexical entries involved in the construction. Apart from the usual hand-waving towards the innate powers of the language faculty, the precise character (and *rational* source) of those constraints was left unspecified.<sup>109</sup> The only forthright claim in this respect was that competence *must* include the ability to rank sentences according to grammaticalness/meaningfulness (despite the vagueness of these notions) and that the two notions can come apart.<sup>110</sup>

I have argued that there are good grounds to think that, on the contrary, grammaticalness and meaningfulness *are* co-extensive notions.<sup>111</sup> I now add the further claim that *there are no fixed boundaries to our semantic categories* (and certainly no *innate* ones).

We could however resist this conclusion from a different perspective than Chomsky's. We could, that is, defend the view that the boundaries of the semantical categories operative in NL are not just inviolable by (some sort of) *stipulation*, but rather that they are fixed by *a priori* laws of *meaning* that discipline derivations and rule out NS ahead of their construction.<sup>112</sup>

This is, as I announced already, pretty much Husserl's view, which could rightly be called *essentialism about meanings*. On this view, the lexicon is formed of basic building-blocks, and the combinatorial laws are written into their essence and normatively constrain usage

Clearly, it is easy enough to concoct any old rule to call out nonsense. What we want is *good grounds* in favour of our specific choice.

<sup>109</sup>Although Chomsky, as we have seen in previous footnotes, toyed with various ideas.

<sup>110</sup>I think it is fair to say that Chomsky did not have any broader aim than establishing these two claims as characteristic of the *cognitive profile* of our competence.

<sup>111</sup>Magidor (2009) makes the claim that even strings that we would normally class as ungrammatical are meaningful, in the sense of: eligible for meaning-assignments. I have no great objection to the view. Unlike Magidor, however, my interest is not (just) in defeating a dogma but rather in finding out *a) why* the dogma has been so resilient and *b) what the facts* about nonsense can teach us with regard to facts about (entertained) content.

<sup>112</sup>Husserl (1900-01/2001: 71) introduced his notion of combinatorial laws of meaning as laws of *nonsense avoidance*, proof, if any was needed, of the crucial role of nonsense in the foundations of semantics.

in virtue of that connection.<sup>113</sup>

Husserl gave no details as to how to implement his suggestion but his view (largely via Tarski 1935: §4) became the building-blocks view that underlies (whether or not in full explicitness) much of contemporary semanticism.<sup>114</sup> In fact, PoC *presupposes* something like Husserl's view, or else there'd be no *properly grounded* base class of meanings to feed into (bottom-up) compositionality, and thus explain CC as required.<sup>115</sup>

The idea then is that we can answer the two questions I raised above in one fell swoop: we prime the atoms by appeal to combinatorial laws grounded in the “nature of things” that fix their properties for *all* occasions of use; moreover, the move is *self-justifying* (the essentialist priming provides appropriate normative guidance both on our choice of selectional rules *and* on competent use).<sup>116</sup>

Essentialism about meanings is just one possible strategy, however, and likely to ruffle any remaining traces of Quinianism in one's philosophy. Let me therefore briefly review some alternatives before I move on to discuss boundary-implementation issues.

<sup>113</sup>See in particular Husserl (1900-01/2001: 62-68). In the same passages Husserl was insistent that meaning categories were not (merely) psychological but fully objective; elsewhere (ibid. p. 11), he had also insisted that conceptual categories are in no way hostage to “mere contingencies of our thinking”. Rather, they reflect (and track) “real differences, grounded in the pure essence of things”. For Husserl, there is a strict correspondence between conceivability and (ontological) possibility (although there is no language/world isomorphism, p. 50). Nonsense is ruled out “*by the very nature of the constituents*” which can only enter into “*definitely constituted meaning-patterns*” (p. 63, my emphases). A good account of Husserl's theory of meaning is Simons (1995).

<sup>114</sup>The building-blocks view (e.g. Frege 1885: 113) is not to be confused with the (Augustinian) building-blocks theory criticised in e.g. Davidson (1964: 4). In the model-theoretic tradition, the crucial fact in this respect is the assumption that the interpretation function matches the *intended model* for NL: quite what that model is (specifically: with respect to the precise contours of the extension of its predicates) is precisely the point at issue; the building-blocks perspective, however, is assuming sharply determined boundaries set *in advance of enquiry*.

<sup>115</sup>Mere stipulation with respect to that class wouldn't do. To guarantee that complex meanings are *properly* determined from the base (that they match our intuitive but well-considered judgments) we require a stronger anchoring than that—indeed, the semanticist thinks we got the basic categories *right*. The Chomskyan framework too presupposes a pre-formed basis for derivational structure, see e.g. Cook and Newson (2007: 250) and the discussion of projection principles in Chomsky (1981: 31).

<sup>116</sup>As Cook and Newson (2007: 65) note, making the grammar sensitive to the way in which subcategories connect to specific constructions is a crucial part of the linguistic project (and not just the semanticist's).



### 2.4.2. Justifying Boundaries

As I said, Husserl, apart from a programmatic appeal to laws of meaning (that were left unspecified) and to some mereological principles (that suggested a meaning-containment view), didn't say much about justification.<sup>117</sup> At any rate, the available options seem fairly clear (and in one form or another they form the basis for most treatments of nonsense).

Firstly, we could appeal to ontology; we could think, that is, that objects come naturally typed. The correct semantic categories, then, are those that respect (or approximate more closely) the natural ones.<sup>118</sup>

The view is not unattractive, but let me register the following concerns: *i*) it is not clear that it can succeed in justifying CC, since we can *always* raise doubts about the *actual* choice of categories (in fact, the semantic categories themselves *have* changed over time and words have shifted across categories);<sup>119</sup> *ii*) the view would still fail *rationaly* to justify the transition from observed (or presumed) ontological categories to their permanent character with respect to unobserved cases (we can always attack the PoC solution to EP from a Humean perspective, that is, for it is unclear that we can *rationaly* read off all future use from *any* feature that we could detect in the building-blocks).<sup>120</sup>

A second strategy would appeal to imagination.<sup>121</sup> Nonsense is inconceivable, on this diagnosis, and hence we justify the choice

<sup>117</sup>Husserl (1900-01/2001: *Investigation III*) draws an ontological distinction between dependent and independent objects which has a precise correlate in the one between dependent and independent meanings drawn in *Investigation IV*. Husserl (1900-01/2001: 74) also gives transcendental reasons for assuming *a priori* laws of meaning that would avoid the dangers of psychologism (recall that the *Investigations* are largely a reaction to Frege's anti-psychologist critique of Husserl's early work in the philosophy of mathematics). The account however remains rather short on detail.

<sup>118</sup>Perhaps this is David Lewis' view, perhaps not. It is a view that stands behind every type-theoretic approach to NL semantics, for obvious reasons. A particularly strong statement of the view by Martin Joos is quoted in Aarts (2007: 18). Surprisingly, at least to me, the view is endorsed even within pragmaticist frameworks that adopt type coercion techniques to "reinterpret" the (alleged) selectional constraints "forced" by verbs (e.g. Egg 2005: viii).

<sup>119</sup>On language change phenomena, see Chambers *et al.* (2002), Brinton and Traugott (2005), Traugott and Dasher (2005), Eckardt (2006) and Cooper and Kempson (2008). The historical interaction between grammaticalisation processes and compositionality is highly instructive.

<sup>120</sup>There are very general worries in this area, of the kind raised by the already mentioned Austin and Putnam, as well as e.g. Field (1973). I think it would be fairly easy to show that arguments about the indeterminacy of theoretical terms can easily be generalised to most (all) terms in the language. But unlike Field (1973: 193), I don't think this is without consequence for standard conceptions of the objectivity of meaning. Specifically, the claim that semantic categories could ever be taken to be absolutely fixed in the manner required by PoCU would become hard to sustain.

<sup>121</sup>An account of this kind is discussed (but dismissed) in Russell's (1940: 230). His preferred account is a non-cognitivist one given in terms of action-guiding potential (ibid. 230-1; 237-9).

of syntactico-semantic categories by aligning them with categorial restrictions on thoughts. Accordingly, meaningfulness is taken to be co-extensive with thinkability, while grammaticality outstrips both.<sup>122</sup> Again, the obvious concerns with this strategy relate to familiar ones about taking conceivability as a general guide to possibility,<sup>123</sup> as well as to the fairly similar concerns in connection with, say, analyticity (with the spectre of the analogue of non-Euclidean geometries looming in the background). We would therefore still lack an epistemic guarantee that our choice of categories is appropriately (and timelessly) anchored.

Thirdly, we could adopt a hybrid strategy incorporating strands from the first two proposals. That is, we could say that NS are not assessable against *any* possible situation/scenario: the representational claim they (appear) to make is (literally) *incomparable*—there is *nothing* (no thing, however construed) quite like what *they say* there is (their content, presumably, is however not *ineffable*, for the kind of nonsense we are interested in is of the kind that respects overt grammaticality; furthermore, the claim made by NS is determinate enough for us to conclude nothing could match it). It's not, however, that NS are necessarily false:<sup>124</sup> rather, they are simply not *eligible* for confrontation with reality (or with the standards regulating our

<sup>122</sup>“What cannot be thought, cannot be, what cannot be, cannot be thought”—Husserl (1900-01/2001: 11). In discussing tonk, Boghossian (2001: 32) endorses (at least) the latter direction. He also takes meaningfulness to require the existence of a determinate way the world could be.

<sup>123</sup>See Gendler and Hawthorne (2002).

<sup>124</sup>In conversation (and in that order), Andreas Stokke, Hartry Field and Derek Ball have suggested that they are. The proposal seems to be that nonsense *qua* representational failure is *counter-sense*, or false in virtue of meaning, as Quine (1960: 229) put it. To this I have two retorts: first, as Russell (1905: 484) grumbled against Frege's treatment of empty terms, I say that this would give no “exact analysis of the matter” (see Kripke 2005: 1017 for discussion). The sense in which NS are necessarily false (if they are so) is clearly different from the sense in which ‘2 is not a prime number’ is necessarily false. And we want our semantics to *explain* that difference (to paraphrase Davidson (1969: 49), we want to know *how NS come to be* false-in-virtue-of-meaning). Which in all likelihood would bring us back to a category-mistake analysis. Secondly, the proposal faces a serious problem with negation. To see why, compare the pairs ‘Carnap is interested in metaphysics’ and ‘Carnap is not interested in metaphysics’ vs. ‘The number 3 is interested in metaphysics’ and ‘The number 3 is not interested in metaphysics’. The sentences in the first pair come out as contradictories, those in the second as contraries. But on this proposal their logical form is the same. And to say that we need external negation for the second pair whereas clearly there is no need for that in the first pair would represent a breach of PoC with no *syntactic* justification (why should negation behave differently when embedded in NS?). Quine (1960: 182; 229) is a prominent example of this (ill-fated) strategy. See Lambert (1968), Routley (1969), Haack (1971), Thomason (1972), Brady and Routley (1973), Bergmann (1977) for discussion. It seems clear that *if* we think there *is* a class of NS we need a three-valued semantics and also, as Chomsky (1965: 158) has noted, a nonsense operator that should display sensitivity to an ‘undefined’ or ‘meaningless’ third value.

thought).<sup>125</sup> Nonsense, on this third approach, is thus a failure not just of imagination but of *representationality*. It is a case where language misfires (words fail to fulfil their *expressive* function).<sup>126</sup>

The problems with this suggestion are, it seems to me, two-fold: *a*) it first acknowledges that there is *something* that NS express (they *do* have a sense), but then *b*) it says that it is a sense that we are unable to make sense of, one for which we lack any criteria for assessment/evaluation.<sup>127</sup> It thus seems as if NS do have a sense (a sense which suffices for the expression of a thought) and yet their sense is one that is *unthinkable* (on this diagnosis, it is not that nonsensical sentences are *devoid* of sense (sense-less): it is that the sense they do possess is a non-sense).

Language, on this view, seems therefore to possess the remarkable (indeed, paradoxical) capability of *actually expressing the unthinkable* (expressing some *thing* which is unthinkable).<sup>128</sup> It thus seems able both to reach beyond the bounds of (linguistic) sense while still remaining within it—on the face of it, a rather contradictory state of affairs.<sup>129</sup>

Fourthly, there is the option we have discussed already, the idea that semantic categories are grounded in conceptual structure (regardless of the grounding of *that* structure).<sup>130</sup> Nonsense attempts to combine incompatible concepts into an *unacceptable* whole;<sup>131</sup> to rule that out

<sup>125</sup> As Camp (2004: §4) notes, we don't want to equate lack of empirical scenarios for truth-value assessment with nonsensicality *tout court* or else far too many sentences would count as meaningless.

<sup>126</sup> The discussion of the C-sentence in Sorensen (2002: §2) seems to favour an approach along these lines.

<sup>127</sup> Witherspoon (2000: 342) raises this difficulty for the Carnapian position on nonsense. The issue of the proper analysis of nonsense has been much debated in the recent confrontation between two interpretive schools in Wittgensteinian exegesis, the British and the American. Key texts in the discussion are Diamond (1991: ch. 2, 3), Witherspoon (2000), Hacker (2000), Conant (2002), Hacker (2003) and Diamond (2005).

<sup>128</sup> See the Schlegel passage quoted in Chomsky (1966: 68). On the notion of impossible meanings see Fodor and Lepore (1999) and Johnson (2004).

<sup>129</sup> I am not discounting the tenability of such a view—much of the interpretive discussion regarding the *Tractatus* agonises precisely over this issue. To label something as 'unthinkable' is nevertheless something that should give greater pause than has been the case (the discussion in Priest (2002: §9.4) is useful). Consider a classic case in mathematics: division by zero. Most textbooks will explain the artificial stipulation for that case by saying "if we try to divide  $n$  objects zero times, we have an operation that makes no sense". The question is: what *exactly* are we describing here? Which operation makes no sense?

<sup>130</sup> Jackendoff (1990: 52).

<sup>131</sup> I'm thinking here of e.g. Peacocke's (1986: 181) notion of *acceptance condition* for a given piece of content.

we are entitled to choose those semantic categories that reflect our intuitive grasp of concepts (those whose possession conditions match our settled judgements on the matter). As for the metaphysical issues underlying our intuitions on this matter, we can leave that to a different branch of enquiry.<sup>132</sup>

In and of itself, this proposal is hard to object to. The critical moving part, however, is how we conceive of conceptual structure (how much *rigidity* we build into it, both in terms of internal and external boundaries).<sup>133</sup> And I contend that there are two consequences stemming from this view that the semanticist ought to consider, neither entirely friendly to her project.

First, that the concerns raised so far would afflict any realistically inclined view of conceptual structure. Secondly, and more importantly, that a commitment to the enforcement of semantic categories by conceptual structure considerations will make the semanticist *answerable to the actual boundaries of our concepts as embodied in our practice*. Should those boundaries turn out to be ineradicably fluid, the semanticist would have to abide by that finding.<sup>134</sup>

In other words, if *our* concepts evolve over time and their time-indexed boundaries are not completely fixed anyway, the nonsense-predicting powers of conceptual strictures will be again limited in their reach and will indeed be largely *ex post facto*. And the point is (although I have no space to argue at length for that): it is a matter of record that our concepts do so change.<sup>135</sup>

Clearly, there is a lot of overlap among the suggestions above. And they all suffer, or so I'd argue, from the same difficulties (i.e. substantially the same difficulties emerge in different guises, hitting the different proposals in relevantly similar places), namely, that *i*) reflection on past practice shows that conceptual/ontological categories *have*

<sup>132</sup>This move however would amount to a denial of ECE, with the consequent loss of purchase on the CC/PoC connection.

<sup>133</sup>I.e. of how much *play* (Derridean *jeu*) we allow those boundaries to have—I'm thinking here of the notions of concept *narrowing* and concept *broadening* discussed in e.g. Wilson and Carston (2007). Bartsch (1998) defends a constructivist view of conceptual structure with which I've much sympathy. The classic, and strongly realist, Peacocke (1992), in contrast, defends a view that suffers, I think, from many of the problems I raise in the text. Burge (2005: 55ff.) usefully sketches the contrast between opposing views of concepts (and understanding) by discussing the mathematical example of the concept of 'limit'.

<sup>134</sup>It seems clear that for any given conceptual scheme we *can* provide a diagnosis of nonsense in terms of conceptual clash. The point is whether we can do so in the forward-looking manner that is *required* by CC.

<sup>135</sup>Again, the case of theoretical terms is particularly telling in this regard. For some of the problems that they pose for compositionality, see Schurz (2005).

changed and that therefore semantic categorisation principles based on those categories would only be relative to a conceptual scheme (and in any case more fluid than what is needed by semanticism) and that *ii*) even if they *were* fixed for all time, tracking them appropriately would inflate the lexicon beyond learnability.<sup>136</sup>

Indeed, I'd argue that all attempts to define meaning-categories in terms of basic building-blocks have to face our third dilemma for the semanticist:

**SD III:** The basic building-blocks can either be rigidly fixed ahead of use or flexible enough to accommodate future use, but they cannot be both, for too much flexibility will deprive them of their ability to rule out NS in advance, whilst too much rigidity will render them unable to *adapt* to fresh contexts. However, a PoC-based explanation of CC requires that they be rigidly fixed, whereas CC itself states that what is remarkable is words' ability to adapt to contexts and our capacity to make sense of that flexibility.<sup>137</sup>

In short, the dilemma is that the conflicting commitments of semanticism require the building-blocks to have contradictory features: nonsense-busting properties (which require rigidity) and context-adapting ones (which require flexibility). I submit that no entity could possess both properties at once and that neither horn of the dilemma would allow the semanticist to respect the constraints imposed by PoC *and* CC—which does show, it seems to me, that the two principles are indeed incompatible.

### 2.4.3. Implementing Boundaries

Let me now give some more specific, albeit still rather general reasons to think that none of the proposals can be made to work in a way that is compatible with the key tenets of semanticism.<sup>138</sup> In this subsection, that is, I am going to assume that some sort of case has been

<sup>136</sup>With regard to *i*), the semanticist might be tempted to appeal to some weaker notion of category, perhaps defined by means of the 'partial signification' relations suggested in Field (1973: 202)—items in each category would refer to tokens of a *range* of candidate types, rather than of a unique type. This would amount to a species of sense-enumeration strategy (type-enumeration, in this case), and it would suffer from the problems which I discuss in the next two chapters. Nor would it help to appeal to the notion of arbitrary object for the type, as per Fine (1983), for again we lack criteria for associating a given range of objects with each type. As for *ii*), see fn. 144, p. 46, and the last bullet point on the same page.

<sup>137</sup>This aspect of CC is explicitly spelled out in Chomsky (1966: 59).

<sup>138</sup>To reiterate: those tenets include a commitment to CC, EPoCU, MDP and UaKTC, plus the learnability constraint.

made to justify a specific choice of categories. I argue that no such choice would be implementable.

Here's why:

- The most natural way to think of semantic categories is in terms of equivalence classes closed under substitution. This however gets things intuitively wrong. First, expressions can be very unstable under language expansion (new expressions create new contexts where substitutivity *for the old categories* may fail).<sup>139</sup> Secondly, the intersubstitutivity criterion would separate expressions that intuitively belong to the same category (classic case: the 'eat/devour' pair).<sup>140</sup> Semantic categories defined by closure under substitution, then, whatever their provenance and grounding, would end up being either impossibly fine-grained (in all likelihood: singleton classes, for we could always come up with a linguistic context in which intersubstitutivity fails) or drawn only relative to a given language strictly conceived (language expansion would not be conservative with respect to the class of semantic categories, that is). This of course would make CC completely uninteresting since categorial features would only be determined *after* observing an expression's behaviour under *all* possible embeddings, including those generated by *new* words added to the original language.<sup>141</sup>
- It is impossible to draw the boundaries of semantic categories without robust appeal to metaphysics, for we need identity criteria for what counts as e.g. an individual (and *pari passu* for all other categories) before we can specify the range of significance of a given predicate.<sup>142</sup> The same is true for all other features spelling out the various subcategorisation restrictions. In short, the required separation between semantics and metaphysics is simply not available. And it would be no escape route for the semanticist to *assume* some such drawing of boundaries is possible. As I have said already: *that* is precisely the point at issue. If

<sup>139</sup>See Hodges (2005: 54).

<sup>140</sup>Hodges (1998: 11).

<sup>141</sup>Perhaps this was what Quine (1953b: 155) had in mind when criticising Strawson (1952) and Russell by noting "the obscurity of the notion of category involved" in type theory and the difficulties both in settling and implementing categorial restrictions. One might of course accept that *gradience* is a feature of categories (see e.g. Muysken 2008: 3ff.), but the problem recurs: what disciplines the relevant aspects of categorial gradience? It might be objected that I am here taking 'language' in too formal a sense (adding new expressions gives rise to new languages). Quite so. But this is exactly what the semanticist commitment to PoC entails.

<sup>142</sup>A point convincingly argued for in e.g. Strawson (1970).

it proves to be metaphysically impossible to draw fixed boundaries for our categories (say, because our concepts are ineradicably vague), then a compositional meaning theory will have to be a very different beast from what is standardly assumed; its very structure, and the way in which it spells out the normative constraints on performance (in terms of *how* the theory models our competence) would have to be radically re-thought from the ground up.<sup>143</sup>

- Finally, and most seriously, and even assuming that the two difficulties just listed can be overcome, the need to *implement* selectional restrictions to rule out nonsense and thus preserve PoCU lands the semanticist straight into the jaws of my SD I, for a lexicon that would be fully equipped to *predict* the emergence of NS across the board would be unlearnable. It would be, that is, too complex for us to master, for the simple reason that the range of contextual embeddings that would need to be considered fully to predict when NS would arise has cardinality strictly greater than  $\aleph_0$ .<sup>144</sup> The purported explanation of how finite minds can encompass infinite (linguistic) totalities thus flounders under pressure from nonsense.

I conclude that the only nonsense-busting answer open to the semanticist at this point in the dialectic is one whose implementation would exceed our computational abilities.<sup>145</sup>

<sup>143</sup>In contrast to my view of these matters, Larson and Segal (1995: 46) argue that a theory of semantic aberrancy lies “outside semantics proper”. They seem to have forgotten that 43 pages earlier they’d said that semantics should tell us about nonsensical anomalies *and their sources*. Szabó (2000: 40) makes the equally astonishing remark that “as far as the semantics is concerned” liar sentences and Chomsky sentences “do not qualify as genuine sentences” at all. Cappelen and Lepore (2005a: ch. 11) also argue against conflating semantics and metaphysics.

<sup>144</sup>Indeed, Tarski (1935: 216) himself had asked whether before drawing categorial boundaries we need to check *all* sentential embeddings for substitutivity failure or whether one case would be enough to determine the profile of a given semantic category. Consider now the following startling admission by two leading Chomskyans: “Ideally, knowing the thematic structure of a given verb is akin to knowing *everything there is to know* about the sorts of constructions where this verb can appear” (Lasnik and Uriagereka 2005: 6-7), my emphasis (see also Chomsky 1981: 31 and Collins 2003: 428). The point is: how much work can ‘ideally’ be doing in an explanation of *our* competence? Chomsky (1972a: 146) had spoken more generically of lexical entries as containing “a complex account of conceptual structure, nuance, presuppositions.” The computational costs of such an account were however left unexplored. Note also that Lasnik and Uriagereka (2005: 4) blithely concede that we *may* discover “a new subcategorization frame” for a verb (roughly, a sentential embedding where a new thematic role is *disclosed*); all we need to do in those cases, we are told, is “add it to the lexicon.” Again, this may preserve *ex post facto* PoC, but it certainly does not address the CC puzzle.

<sup>145</sup>Every suggestion in e.g. Chomsky (1965: 111) involves *listing* the ways in which lexical features

The upshot is that the attempt to salvage the full generality of EPoCU from the problems posed by nonsense requires the imposition of restrictions on meaningful derivations that force the semanticist to sacrifice learnability (that's my SD I again, and that shows once more that PoC and CC are incompatible requirements on a semantics).<sup>146</sup>

If I am right about all this, the only other option for the semanticist is to argue that we *do* understand nonsense, that the meaning of NS is compositionally determined (and compositionally understood) just as in normal cases. This would clearly preserve the desired CC/PoC connection. But there are other costs involved (it's the other horn of my SD II after all) and in the next section I try to assess them.

## 2.5. Understanding Nonsense

It is now time for some more stock-taking as we move towards the end of the chapter.

We have been testing the joint claims that linguistic understanding is unbound and that compositionality is the best explanation of this fact. I have challenged the first claim using grammatical nonsense as a class of expressions where understanding breaks down *by the lights of semanticism*.

I have then argued that this removes the explanatory connection between PoC and CC—if the language faculty (or the compositional module) does not deliver understanding in the case of nonsense, it is obscure why it should do so in general, given that, as I hope to have shown, it is wrong to say that it is *only* with “normal” sentences that

restrict combinatorial possibilities. The idea that we *can* compile such a list in advance of embedment is of course problematic. *That*, after all, was the problem of explaining CC. And PoC only *assumes* that we have a solution. It presumes that our rules can provide for all unobserved cases, that we can *rationaly* project to all unconsidered scenarios, but all of this is precisely what we had to establish in the first place. Pustejovsky (1995) and some of the contributions in Copestake and Briscoe (1996) attempt to provide a more flexible implementation of lexical restrictions whereby the lexicon *generates* possible combinations *co-compositionally*. These proposals however simply replace lists of features with lists of *sets* of *sub-features*. They thus shift the structural problems one level up. For criticism of the generative view of the lexicon see e.g. Fodor and Lepore (1998), Geeraerts (2010: §4.1.4), Kilgariff (2001), Asher and Lascarides (2001) and Jayez (2001).

<sup>146</sup> Another way of putting it is to say that after at least Chomsky (1970a) the leading idea is that lexical items contribute *all* their information to derivations; this, in fact, entails a massive amount of idiosyncrasies being pumped into *any* derivation. We could screen off redundancies by going for some kind of default/non-monotonic logic account of meaning, but then normality presumptions kick in, completely by-passing the assumed compositionality of meaning: if so, *why* go via the detour of compositional computation? Why not say that we simply ‘buy’ the pre-fab units of interpretation, as discussed in e.g. Dąbrowska (2004: §3.1).



the compositional machinery furnishes us with pre-formed meanings (conversely: that it is only with nonsense that it *demand*s interpretive contributions from speakers).<sup>147</sup>

I have then examined ways in which the semanticist could explain failures of closure with respect to understanding by appeal to categorical restrictions that would predict the emergence of nonsense (understanding breaks down when illicit meaning-derivations are attempted).

My conclusion in this regard has been two-fold: it is not clear that a sufficiently strong justification (and certainly not one given on *a priori* grounds) is available for any particular choice of semantic categorisation (whether led by syntax or by conceptual structure); moreover, any such choice would entail inflating the lexicon beyond learnability.<sup>148</sup>

Saving PoC as the only explanation of CC, then, would lose the semanticist her other key thesis, namely, that compositional languages (and indeed, only compositional languages) are learnable.

Throughout, a large dilemma loomed (which I labelled SD II): the semanticist can either say we understand nonsense or that we don't. So far, I have considered the second horn and concluded that it forces the semanticist to abandon (at least) one of three jointly-incompatible theses, CC, PoC, and learnability.<sup>149</sup>

I hope I have said enough to show the force of the second horn. I now want to turn to the first horn of the dilemma. I will argue that it makes for equally uncomfortable seating.

### 2.5.1. Representationalism, Again

Suppose then that the semanticist has conceded that my case against the first horn is sound; suppose she has granted that accepting Chomsky's notion of grammatical nonsense and saying that we do not understand it is not a viable option for her position. Still, the semanticist will want to resist my conclusion that PoC provides *no* explanation of CC.

The obvious (and I think the *only*) way to do so is to say that we *do* understand nonsense (that nonsense is indeed meaningful), and that

<sup>147</sup>Let me be brutally Nietzschean: so-called normal sentences are nonsense about which one has forgotten that this is what they are.

<sup>148</sup>Here's another casualty of nonsense: Horwich's (1998: 154) claim that the compositional machinery is insensitive to the precise constitution of the lexical properties.

<sup>149</sup>The precise nature of that incompatibility depends on the specific formulation of the three theses.

accordingly our understanding *is* unbound (just as CC says) and best (only) explained by the compositionality assumption.<sup>150</sup>

Given EPU and UaKTC, to say that we understand NS commits the semanticist to saying that we know their meaning (by EPU), that is, that we know their truth-conditions (by UaKTC). And that seems to encase the representationalist leanings of semanticism quite aptly (knowing the meaning of a sentence is knowing the representational claim it makes) and in full generality.<sup>151</sup>

Consider now a much-discussed case of semantic aberrancy:

2) Max cut the sun<sup>152</sup>

In introducing the example, Searle commented that it poses a problem to compositional theories because although we understand the single words, we don't understand the sentence as a whole, and hence we do not really know what the truth conditions determined by the meaning of the sentence are supposed to be like.<sup>153</sup>

Mirroring Chomsky's remarks, Searle argued that without the provision of a context, (2) all by itself does not provide us with an interpretation (in logic-speak, we would say there is *no intended model* for it).<sup>154</sup>

On Searle's view, there is such a thing as the literal meaning of a sentence, but it only determines truth conditions against "a set of background assumptions and practices" (p. 227). Two further claims by Searle are of interest to us: that what conditions a sentence determines is always relative to a given set of assumptions and practices (what he calls 'the background'), and that the background is not part

<sup>150</sup>Could we say that the understanding of the complex is *precisely* a function of the understanding of the atoms? The idea is: any dark area in our understanding of the atoms will transfer to our understanding of the complex—e.g. we do not *fully* understand 'green' until we *have* made sense of 'green ideas'. Compositionality is thus fully preserved. I cannot see how this could solve our difficulty though, for it would become unclear how bottom-up compositionality could explain CC. PoC would therefore be an *ex post facto* principle as far as linguistic understanding goes. Williamson (2003b, 2007) has argued that understanding is a very complex capacity that cannot be reduced to a uniquely specified set of abilities. Quite. But this is unwelcome news for the semanticist. Another option is to make recourse to Burge's (2005: 56) notion of incomplete understanding (see also Ziff 1972: 5, Higginbotham 1989: 156 and Buekens 2005: 71). We could then say that we understand NS only partially. The difficulty here would be that with nonsense there seems to be nothing *further* to understand. The understanding is *ex hypothesi* terminally incomplete.

<sup>151</sup>In fact, it seems a widely assumed claim: meaninglessness is often defined in terms of lack of representational power. See e.g. Boghossian (2001: 33) for just one example.

<sup>152</sup>Searle (1980: 225).

<sup>153</sup>Strictly speaking, understanding breaks down at the stage in the derivation where the complex predicate 'cut the sun' is formed.

<sup>154</sup>Unlike Chomsky, Searle generalised the remark to all sentences.

of the semantic content of the sentence.

I think that Searle's move (which I largely endorse) should be seen as an attack on the Myth of the Given in its linguistic form, an attack on the idea, that is, *that sentences could self-intimate their meanings*, that, as Chomsky put it, *they* (can and do) impose an interpretation on us.<sup>155</sup>

As it happens, the semanticist reply has for the most part concentrated on arguing that Searle and his followers conflate *evaluating* sentences with understanding what they say, that the conflation obliterates a fundamental distinction between knowledge of truth-conditions and knowledge of whether those conditions are satisfied.<sup>156</sup> Knowledge of meaning is knowledge of (and hence understanding of) truth-conditions only, and *that* knowledge does not have to include knowledge of their verification procedure, we are told.

To require more than that, the semanticist argues, is "to fall prey to a kind of creeping Verificationism."<sup>157</sup> On this line of response, the semanticist is happy to bite the bullet and claim that in a case like (2) it is not clear "that knowledge of the meanings of the parts of the sentence and their mode of composition does fail to add up to knowledge of the condition under which [...] an utterance [of (2)] would be true."<sup>158</sup> Competent speakers, that is, will know that (2) is

<sup>155</sup>Note how the very way in which Chomsky puts it *violates selectional restrictions*! Sentences, inanimate objects, *imposing* interpretations on sentient subjects? Whoever heard of such deviancy! For his part, Searle (1980: 231 and 1983a) includes intentionality within the scope of his argument: contextual dependency is ineliminable for content in general, not just semantic content. I agree entirely. I depart from him in denying that the background can play the *sole* role of meaning-determiner. Frame semantics in the Fillmore tradition and meaning-constructivism more generally include further factors, such as speakers' 'creative' input in *selecting* interpretations. See for instance Coulson (2001: 81). One could of course include speakers' input in the background, but then Schiffer's (2003: 122) objection against the inclusion of intentions in semantic clauses kicks in (intentions make *everything else* in the clauses redundant).

<sup>156</sup>According to Recanati (2004a: 92, fn. 22), appeal to this distinction seems to originate with Marcelo Dascal in a 1981 paper.

<sup>157</sup>Borg (2004: 238). The same accusation is made in Cappelen and Lepore (2005b: 57). Jackson (1998: 74) argues that "understanding does not require knowing the proposition expressed". His main thought (p. 72, fn. 26) seems to be that, in Stalnaker's terminology, "understanding requires knowing the propositional concept associated with a sentence, though not necessarily the proposition expressed" or, in Kaplan's terminology, "understanding requires knowing character but not necessarily content." I think this move, though obvious, is hopeless when applied to NS, for it is utterly obscure how we could properly be said to know a function-like entity (character) that supposedly *determines* a value-at-a-point-of-evaluation (content) without knowing (even in principle) what that value is (and how we *could* calculate it)—analogies with the classic cases ("He's a fine fellow") or the Ackermann function would not help: *pace* Lasersohn (2009), the meaning function *must* be effectively computable by *us*.

<sup>158</sup>Borg (2004: 236).

true iff Max cut the sun.<sup>159</sup>

Emma Borg is a prominent defender of this strategy and her key defensive move is the claim that semantic theories provide *liberal* truth conditions—conditions, that is, that unlike their more strict Fregean counterparts “admit satisfaction by a range of more specific states of affairs.”<sup>160</sup>

Grasp of meaning, on her view, is thus grasp of liberal, rather than robustly Fregean, truth-conditions. And we may grasp a condition without being in a position to grasp what ‘more specific state of affair’ is at play in a given context.<sup>161</sup>

Once this concession is on the table, it is open to the pragmaticist to reply that it is not at all clear *what exactly* knowledge of a condition that we are apparently in no position to evaluate can amount to.

As Travis never tires of pointing out,<sup>162</sup> the traditional semantic project claims to be in the business of establishing a *uniquely determined* connection between meaning and truth and yet we are now told by the semanticist that the one thing T-theorems *cannot* do is give conditions for the *truth* of sentence-tokens, but can at most indicate a (possibly vague) set of such conditions. The fact is, embracing liberal truth-conditions amounts to jettisoning MDP outright.<sup>163</sup>

<sup>159</sup>Unhappily for her, she adds: “that is, just in case [Max] stands in *the* cutting relation to the sun” (my emphasis). Unhappily, for it is completely unclear what *that* relation would amount to. Similarly, Buekens (2005: 71) has argued that “it does not follow from the fact that we do not see in what kind of circumstance a sentence could apply - i.e. could be used as a true sentence - that we do not understand the sentence”. So much for the semanticist’s core principle that meaning and truth team up to establish a *unique* connection between sentence-in-context and world and that knowledge of meaning is knowledge of that uniquely identified representational claim. The difficulty for the semanticist is familiar from the discussion in Evans and McDowell (1976) and LePore (1986a): what is required for language mastery is not knowledge of the truth of the meaning-conferring bi-conditionals (knowledge-that), but rather knowledge of the truth of the R.H. side (knowledge-what). See Salmon (2001) for further discussion. Devitt (1997: 271) defends a rather strange conception of knowledge-what.

<sup>160</sup>Borg (2004: 230). I think the defence of semanticism in Predelli (2004, 2005) is vulnerable precisely along the same lines discussed in the text.

<sup>161</sup>Borg’s position could be seen as a version of what Levinson (2000: 240–41) has called ‘semantic retreat’, namely an impoverished view of semantics whereby all that the compositional machinery outputs is relatively schematic logical forms. On this view, most interpretive matters are left to pragmatics.

<sup>162</sup>See the essays in Travis (2008). The first 11 pages of Travis (2006a) will do as an introduction.

<sup>163</sup>I doubt it would be much help to do as Atlas (2007: 218) suggests (possibly tongue-in-cheek) and maintain that ‘determine’ in MDP is best understood as ‘constrain’ (see also Soames (2005a: 274) for a similar suggestion). That however is something that most pragmaticist would be only too happy to grant (see also the discussion in Clapp (2007a,b) and Borg’s (2010) reply). The problem for the semanticist is that liberal truth-conditions are both too strong and too weak: they are too strong in that they require that a sentence determine a *unique* such set; and they are too weak in

Furthermore, it is also unclear why (and how) appealing to the distinction between knowledge of truth-conditions and knowledge of verification methods would help.

Unless I know *which* condition Max has to satisfy for (2) to be true, I'm in no position to evaluate *whether* he's actually done so. That is, until we are told *what* cutting the sun amounts to, we are not in a position to understand not just what would count as satisfying that condition, but also what that condition *is*, *what it is* that we are supposed to *cognize* when we reach the end of our compositionally-led meaning computation for the sentence.<sup>164</sup>

More generally, linguistic competence is competence in *using* pieces of linguistic lore (deploying them, reacting to them).<sup>165</sup> A model of that competence that does not provide clear 'instructions for use' looks like a miserable failure, as models go.<sup>166</sup> And although knowing a condition and knowing that it is satisfied are two different things, we cannot know a condition without knowing what would count as satisfying it; that is, there's no giving a condition without stating its applicability conditions.<sup>167</sup>

For illustration, consider the case of a speaker put in front of a strikingly novel scenario. Something's happening to the sun; Max, the mad scientist, is doing something to it. Does *that* count as cutting the sun?<sup>168</sup>

On Borg's account, it is the semantics that provides the material that *which* specific condition applies is left unspecified—see Borg (2004: 245).

<sup>164</sup>The distinction between knowing-what and knowing-whether is well present even to a justificationist such as Dummett (2006: 48-9). By the semanticist's own lights, however, grasp of sense is first and foremost grasp of knowing-what.

<sup>165</sup>The semanticist will insist we must not conflate competence with performance, meaning with use—this is in effect the charge made in Katz (1981) against Searle (1980). But the point remains that if the semanticist insists on too strong a separation between competence and performance, most of her claims about the (supposedly *explanatory*) role of the competence-modelling structure will not be justified.

<sup>166</sup>Could we say that a competent language user will know one thing for sure, namely, that there are no 'safe' conditions for the use of nonsensical sentences? There's a problem with this reply: for it seems to differentiate between knowledge *of* meaning for the standard cases and, in aberrant cases, knowledge *that* meaningfulness is defective.

<sup>167</sup>To make the problem for Borg's position even more perspicuous, consider the case of vagueness. As Wittgenstein (1932-35/2001: I.III.41 p. 83) notes, it is a criterion for one's being in a position to understand the expression 'red' that one be able to pick red objects on demand (if one cannot, then one doesn't know the *semantics* for 'red'; one *doesn't understand what one is talking about*). And similarly for 'cut the sun'. Incidentally, the substance of my objection to Borg is pretty much aligned with Wright's (2002a: 97-8, fn. 2) against epistemicism about vagueness.

<sup>168</sup>Here and elsewhere, use of 'count-as'-language should not be taken as strictly weaker than use of 'is'-language.

for a full understanding of the expression (recall: Borg states the requirement as a *bi*-conditional!); what counts as cutting the sun in this specific (and unprecedented) case is supposedly *already* part of the set of liberal truth-conditions.<sup>169</sup>

This, I contend, seems hardly plausible.<sup>170</sup> The intuition (if not the evidence) seems to be, rather, that a speaker *in a genuinely novel case* would have no clue *from the semantics* as to whether the predicate does or does not apply to the case.

There is a possible retort here, and one that Borg (2004: 234, fn. 28) has made. We could say that e.g. the precise way in which one judges whether or not a certain event counts as a token of sun-cutting is a matter of *worldly* knowledge, rather than purely linguistic knowledge (and thus it does not belong to linguistic semantics proper).

The move is inadvisable, however. For it consigns the semanticist to the jaws of yet another dilemma:

SD IV: If we could neatly divide the worldly and linguistic knowledge that attaches to expressions and insist that PoC only governs linguistic knowledge, our semantic theory would no longer explain CC (for we genuinely understand a sentence *only* when we are in possession of the appropriate worldly knowledge).<sup>171</sup> If on the other hand we could not separate worldly and linguistic knowledge, the lexicon would become unlearn-

<sup>169</sup>Liberal or not, the semanticist picture, as Wittgenstein (1914-16/1998: 23) noted, requires that we know *in advance* how things ought to be like for a sentence to be true. Remove this requirement, and the position ceases to be recognisably semanticist (indeed, without it, we couldn't even effect the transition from knowledge-what to knowledge-whether). I think recent defences of semanticism, such as Whiting (2007a: 309), are still fully committed to a requirement of this kind. Borg (2010: 36) has further retreated to the weaker requirement that liberal truth-conditions determine application conditions for "clear-cut [!] cases". My argument re the C-sentence applies just as much to her latest retrenchment.

<sup>170</sup>Saint-Dizier (2001: 116-17) has suggested that a verb such as 'cut' can be characterised in terms of lexical features that contain domain-dependent predicates in the qualia structure (to cut is to implement a change in some object from a continuous to a discontinuous aggregation of matter). Note that the interpretation of the predicates in the qualia is intended to be context-dependent! As I've argued already this simply adds an additional layer of complexity— why not interpret 'cut' directly without the detour through the additional features which simply duplicate the original problem?

<sup>171</sup>Again, note that it is NS that show this to be the case. Under the proposal we are considering, we understand NS and yet have (little or) no idea about their worldly import. Am I begging the question here? I don't think so, for the very conception of liberal truth conditions encompasses the range of *all* possible circumstances in which a sentence would be true. And *that* is a (rather large) piece of worldly knowledge if anything is. Besides, the central point about CC is that it claims that we understand novel sentences *robustly*, we know exactly *what* they are talking about. And by the semanticist's own lights that means: we know *the* representational claim they make (or, on Borg's view, the range of such claims). Let me stress once again that on the semanticist picture the representational claim made by a sentence is part of its *semantic* content (Skorupski 1993: 149).

able, for reasons already discussed.<sup>172</sup>

Liberal truth-conditions, I conclude, are no friend to the semanticist.

### 2.5.2. Scenarios

Apart from (or in addition to) appealing to the notion of liberal truth-conditions, semanticists could also insist that aberrant sentences are meaningful—indeed, that they have *precisely* the compositional meaning that they appear to have—by saying that we understand *enough* of what they say to be able to work out scenarios where they *would* be true.<sup>173</sup>

This reply however would be just as unwise. Invoking imaginative capacities as *the* way to work out when an aberrant sentence would be true plays entirely in the hands of the radical contextualist.

The reason is that it seems obvious that an appeal to these capacities *a*) could not be contained to nonsense cases but would have to be extended to all the Travis-like cases (i.e. green leaves and round balls) because of the (intensional!) instability of (as good as) all predicates (and thus of *all* (atomic) sentences); *b*) the move would make knowledge of meaning *insufficient* for understanding: we simply wouldn't *know* what the truth conditions given on the R.H. side *say* in the absence of a *decision* about *i*) *which* scenarios would count as relevant and about *ii*) *which* of those scenarios would count as making an utterance of the sentence *true*; and finally *c*) it would make *linguistic* competence an ability that perforce invokes cognitive faculties wider than the language faculty (it's that imaginative rationality yet again).<sup>174</sup>

Moreover, as stated already, the main motivation for compositionality, the CC, would lose any force whatsoever, for on this proposal we don't (fully) understand nonsense (or *any* novel sentences) *until*

<sup>172</sup>Kaplan (1990) has an instructive take on these matters. My own view is that the purported separation is unworkable and does violence to *semantic* facts. Kilgarriff (2007: 38) is useful here, as are Hudson (2007), Langacker (2008: 37), Taylor (1989: ch. 8) and Green (1996: 18). I think that what nonsense shows is two (apparently contradictory) things: that what we understand is radically minimal content; that what we learn when we become competent with an expression is inextricably linked to world knowledge. Chapter 6 proposes a resolution of the tension.

<sup>173</sup>A point made by Torfinn Huvenes in discussion. Talk of scenarios and situations is invoked in Recanati's (2004a: 15) discussion of intuitions about truth-conditions. Buekens (2005: §6) develops a semanticist proposal where sentences-in-use are seen as playing the role of epistemic substitutes for situations.

<sup>174</sup>And again, this would play entirely into the hands of the radical contextualist, as the arguments contained in Searle (1978) and Putnam (e.g. his 1975b: 243-44) would just be granted full sway.

*we have fixed a range of scenarios that would match the metalanguage description of the truth conditions.*<sup>175</sup>

The problem however was precisely that we *lack* a grasp of those conditions *at the metalanguage level*. And the semantics leaves it unconstrained *which* range of scenarios is relevant (there is *nothing* in the *current* meaning of ‘cut’ that tells us anything substantive about what it would be like to cut the sun).<sup>176</sup>

If so, there is then no robust sense in which the range of scenarios can properly be said to be given in advance of a *specific* confrontation with reality; and we are not given a recipe for applicability once faced with a particular scenario either. The final *decision* is left to appropriate exercise of our imaginative rationality (a problem I shall return to in the last chapter).

In short, if in response to NS the semanticist appeals to scenarios, she is conceding that we do not *fully* understand novel sentences after all, and if so, she is facing again the other horn of my SD II. It then becomes unclear why she should insist that NL semantics *must* be compositional.<sup>177</sup>

But then, if the creativity considerations drop out of the picture, if *meaning* (rather than understanding) is indeed unbound and its (fairly indefinite) extensibility is generated (and controlled) by the expansion of the range of what *we* take to be acceptable (because imaginable) scenarios, what remaining reasons do we have for insisting on the compositionality requirement and, relatedly, on the UaGS view?<sup>178</sup>

If the considerations in this section are sound, it looks as if the se-

<sup>175</sup>Clearly enough, what would go into that range seems largely a function of time-indexed worldly knowledge, rather than syntax-governed competence.

<sup>176</sup>And short of believing in rampantly Platonistic meanings, we should grant that *if* (and only if) *we were to witness an event of sun-cutting*, we would *thereby* be determining a *new* concept (assuming we live to tell the tale, of course).

<sup>177</sup>There’s a confusion, or so it seems to me, in some standard views about semantics, even as stated by the most celebrated and accomplished of commentators. Soames (1989: 591), for instance, concludes that “[t]he central semantic fact about language is that it is used to represent the world. Sentences do this by systematically encoding information that characterizes the world as being one way or another. Semantics is the study of this information, and the principles by which it is encoded.” But this seems to attribute to language powers that *no* system of sign could possibly have. There’s no information that, *on its own*, can characterize the world as being one way or another.

<sup>178</sup>There are already arguments against the creativity considerations in the literature that are independently motivated, e.g. Baker and Hacker (1984: ch. 9), Schiffer (1987: ch. 7) and, less convincingly, Matthews (1986). I’m arguing here that nonsense provides further grounds for abandoning this supposedly platitudinous explanation of creativity. In passing, I discount probabilistic accounts of meaningfulness (such as e.g. Bod 1998: 3). I take Chomsky’s (1955-56/1975: 145) arguments against statistical approaches to be definitive. Indeed, that was the whole point of his C-sentence!



manticist is, once again, in a bit of a bind.

If she insists that aberrant sentences are (minimally, or homophonically) meaningful, she is restricting too severely the powers assigned to what on the semanticist view (and perhaps the common sense view as well) *should* count as meaning, with the further consequence that the sense of understanding at stake becomes etiolated beyond comfort.

At a minimum, she is certainly severing the crucial connection between understanding the meaning of an expression and exploiting *that* understanding to establish the truth-value of that expression—there is no way in which the semantics can properly be said to *determine* the *content* of the sentence (the precise stipulations for the truth condition).

As a consequence, there is also no remaining way of construing the sentence as *representing*, in any sufficiently robust manner, the worldly conditions that have to be satisfied for the sentence to be true.

In short: saying that nonsense is meaningful is tantamount to saying that meaning doesn't fully determine truth-conditions robustly conceived. Accordingly, one of the hallmarks of semanticism has to be given up.

If on the other hand the semanticist appeals to the availability of scenarios (and lack thereof) as the decisive criterion for meaningfulness (so that aberrant sentences would count as *partially* meaningful, but in any case sufficiently informative to guide judgement *should an appropriately relevant scenario become available*), she is then moving her position too far to the contextualist side of the debate.

For, in her scenario-reliant guise, she cannot sensibly insist that whatever minimal meaningfulness accrues to the aberrant sentences from the bare calculation of the semantic values of their parts *suffices to determine*—in all possible scenarios, including, *ex hypothesi* completely *unsuspected* ones—what would count as *the* correct (i.e. rationally mandated) judgement to make regarding their truth value given those novel scenarios.<sup>179</sup>

In fact, it even becomes hard to say that (character-level) meaning *constrains* the determination of content, because what matters now is what *we* would judge to be the rational/natural thing to say regarding the truth value of a sentence given a certain scenario, rather than what

<sup>179</sup> Appeal to ontological constraints of the kind invoked by Lewis (eligibility, naturalness of properties) would not serve much purpose either. Consider Jakobson's 'pigeon's milk' example (quoted in Margalit 1978: 384). Its non-existence does not impinge on the significance of the expression. Ontological irreality and senselessness, Jakobson concludes, are not to be conflated, and it seems hard to disagree—actually, there *is* something called 'pigeon's milk'. It's not milk, but is secreted by pigeons (regurgitated food of some kind); Jakobson's remark is in any case well-taken.

the semantics tells us to do.<sup>180</sup>

The last remaining horn is thus just as uncomfortable as all the others. I therefore conclude that nonsense does indeed pose terminal problems to the PoC/CC connection and mandates a revision of our conception of semantic content. My work in the next chapters will largely be devoted to preparing the ground for such a revision.

## 2.6. Conclusion

I have argued that two of the most crucial semanticist commitments are not jointly satisfiable: a proper explanation of how we understand novel sentences is incompatible with the requirement that complex meanings be functionally determined from below.

I hope to have shown that CC and PoC entangle semanticism in a web of dilemmas that weaken the position beyond recognition. In essence, the problem is that the explanatory burden posed by CC saddles semanticism with epistemic duties that cannot be fulfilled as long as the parallel commitment to PoC (and MDP) remains in place.

Before we move on to the next chapter, let me collect a few more strands from the preceding discussion, so as to give you a flavour of where I am heading.

### 2.6.1. The Road Ahead

I do not claim to have shown that meaning is everywhere provisional and judgement-dependent,<sup>181</sup> although I could say (provocatively) that there actually is a very good sense in which it *is*—in language, it is familiarity that breeds content.<sup>182</sup>

<sup>180</sup>The slash here is doing a lot of work. More about this contrast in chapters 3, 5 and 6.

<sup>181</sup>I take it that phenomena such as *meaning without use* and *meaning illusions* do show that certain structural elements are fixed independently of whatever anyone thinks (see e.g. Hinzen 2006: 153). There are e.g. familiar cases (often involving negation) where we are (systematically) deluded about certain constructions that strain our computing powers. Classic cases: “no head injury is too small to ignore”; “I cannot stress enough the importance of *X*”. There is widespread (but not universal!) agreement that speakers are commonly mistaken about the meaning of these sentences. Note however that verbal illusions concern cases where competence and performance come apart. Nonsense, by contrast, concerns cases where competence and performance go hand in hand *in stretching the boundaries of our conceptual scheme*. I would therefore argue that nonsense and verbal illusions seem to show that a single, fully general account of the mechanisms that generate complex meanings cannot be given. Compositionality, that is, displays what linguists call *semi-productive* features (and the technical difficulty is finding ways to implement that notion while saving all the phenomena).

<sup>182</sup>You could see the bulk of my argument against Chomsky in section 2.3.3 as a challenge against the allegedly privileged status of “normal” sentences and the related immunity from the persecution

Here, however, I limit myself to arguing that meaning is not *in general* compositionally determined in the manner assumed by semanticists and that CC provides little reason to insist that a semantics respect PoC.<sup>183</sup>

Above all, nonsense shows that it is demonstrably false that “syntax carve[s] a path that interpretation must *blindly* follow”.<sup>184</sup>

My suggestion, then, is that complex meanings, if conceived *à la* semanticist (that is, as something that determines the conditions for the truth of embedding sentential expressions), are *in principle* provisional, that compositionally-determined meanings require validation by speakers before the *range* of situations that would make them true can be fixed.<sup>185</sup>

Similarly, the important lesson from nonsense, I think, is that novel meaning-derivations issue in genuinely new concepts and that therefore PoC cannot provide an empirically adequate explanation of CC (it is false *that* we understand novel sentences in the immediate fashion posited by the semanticist and the way we *do* robustly understand them—eventually—is mediated through processes that exceed the powers of the language faculty).

In fact, when a genuinely new complex meaning is formed, a *new* meaning for the constituent senses involved is thereby determined. It is *our* grasping a new proposition that determines the new atomic meanings that we obtain via *de*-composition.<sup>186</sup> In this respect,

of use that Chomsky seems to grant them.

<sup>183</sup>By analogy with the notions of degrees of grammaticalness and degrees of nonsense, one could perhaps retreat to a notion of *degrees of compositionality*, and indeed there are various proposals of this kind in the literature (e.g. Costello and Keane 2005). However, I doubt that a semanticist could make this concession while holding on to the MDP, for espousing a view where PoC has only partial applicability usually entails a commitment to weaker claims with respect to meaning determination—see e.g. Langacker’s (2008: 245) talk of compositional *prompting*.

<sup>184</sup>As Uriagereka (2002: 275) put it (my emphasis), cited approvingly in Hinzen (2006: 250), Chomsky (2006: 15) and Uriagereka (2008: xxii) himself. This is one point on which Chomskyan minimalism misspeaks itself.

<sup>185</sup>I find much of the discussion in Williamson (2007: ch. 4) congenial to my aims, in particular the contention that there are no firm criteria for applicability conditions (p. 123–24), that even so-called analytic truths may later be called into question (p. 126) and that the notion of understanding is too unstable to do much explanatory work. What I find puzzling is Williamson’s continuing enthusiasm for the achievements of truth-conditional semantics (pp. 281, 285). Just as Williamson notes (p. 133), thin conceptions of understanding afford very limited explanatory powers. Accordingly, it is time we severely toned down the claims as to the reach of semantics standardly made.

<sup>186</sup>Am I granting some form of reverse compositionality then? Yes and no. For what I am challenging is the requirement that the base class of atomic meanings predate the class of complex meanings. There are interesting parallels with set-theoretical questions about the explanatory (and ontological) priority of elements over their containing sets. *Contra* Davidson (1967: 26), Goldberg (1995, 2006) defends the thesis that certain syntactic constructions make a specific contribution to

PoCU gets things *exactly* back to front.<sup>187</sup>

Furthermore, I think it would be severely misguided to suggest that we are merely *discovering* (or unveiling, or shedding fresh light over, or whatever other metaphor the realist wants to invoke) an already pre-formed meaning determined by the “nature of things”.

If you prefer to think of the matter in Fregean terms and say that by means of conceptual analysis (prompted by our encounter with novel meaning-combinations) we refine *old* concepts, that we improve our rational insight into their structure, and that meaning-derivations therefore disclose a *Dritte Reich* of *independently formed* pieces of content, well, by all means do so.<sup>188</sup>

I think that this however would amount to no more than uttering a battle-cry, as *Zettel* §414 should have taught us long ago—and the history of mathematical concepts provides plenty of evidence that realism about these things is on flimsier ground than expected even in the most hallowed enclaves of rationality.<sup>189</sup>

For the very same reasons, I have argued that the class of grammatical nonsense is empty. Just as *Tractatus* §5.4733 had warned against Frege, every grammatical sentence *must* make sense and reflection on nonsense brings to the fore the point that it is *impossible* to clash meanings together.

Nonsense as combinatorial catastrophe, that is, is a nonsense position to take: try as you may, you *cannot* form nonsense by clashing categories, for this assumes that signs can have a referential rigidity

sentential meaning that is *not* traceable to either the constituents or to standard modes of combination (see also Millikan 2005: 199). Jackendoff (1990: ch. 10) keeps PoC by speaking of “constructional idiom[s]”. If Goldberg is right, however, there is (at least) a *third* level involved in the determination of complex meanings and PoC does not tell the whole story with respect to CC.

<sup>187</sup>The idea here is that we grasp novel meanings by first grasping (*in a flash*) the proposition that a sentence is expressing. We then de-compose it into its constituents, thereby creating (possibly new) constituent meanings. The class of complex meanings is however not closed under recombination, as far as our understanding goes—recombination is in general non-conservative. It was after all Frege (1919: 25) himself who said that we “come by the parts of a thought by analysing the thought” and not viceversa. Quite how we should reconcile that with Frege (1914: 225) is a matter for another discussion.

<sup>188</sup>See the discussion in Burge (1990: 259).

<sup>189</sup>Gauss’ view about the notion of function is illuminating: “One should never forget that [...] functions [...], like all mathematical constructions, are only our own creations, and that when the definition with which one begins *ceases to make sense*, one should not ask ‘What is it?’ but ‘What is it convenient to assume in order that it *remain* significant?’”—cited in Kline (1972: 1032), my emphases. I think that is *exactly* the right attitude to have concerning NS. And the very concept of number, as again Wittgenstein has extensively argued, is similarly hard to construe realistically. Did the acceptance of the notion of complex number not *modify* our concept of number and determine a radically *new* one? I cannot see any sane way of arguing otherwise.

that no sign could possibly possess.<sup>190</sup> In the next chapter I shall return to this issue too.

### 2.6.2. What Role for Structure?

Are there then *no* remaining reasons to insist on structural (specifically: compositional) constraints on linguistic competence?<sup>191</sup> Well, yes, of course there are. They do not however concern the need to avoid case-by-case meaning stipulations, or the (supposedly transcendental) point about making sense of the phenomenon of shared content.

Rather, (some form of) compositionality (and UaGS) is needed to ensure that our system of signs is a *language* and not a *code*, that expressions *express* content and not merely point towards it (or are *paired* to it).<sup>192</sup>

That's no small concession, of course, and by acknowledging the legitimacy of this theoretical demand it may seem that I am committing myself to an enormous explanatory task, namely, that of reconciling a distrust in strict compositionality with the recognition of its indispensability in ensuring that our signs function as parts of a language

<sup>190</sup>If this leaves you unconvinced, go back to my discussion in section 2.4.2. The idea that we could form a sense that expresses unthinkable thoughts is the one we are asked to make sense of. I think *this* idea is nonsense. One could retort that *the very idea of language change* requires that categories *can* clash—or else, how could we account for the current *acceptability* of (1)? Again, I think this is a misunderstanding of the facts. At no point were we entertaining the thought of (a putatively literal, and thus *still abhorrent* reading of) 'revolutionary' combined with 'ideas'. As soon as the pairing was coined, we *saw* the intended meaning (our imaginative rationality delivered us the "new" meaning). One could still complain that I am flattening useful distinctions here: children delight in Alice-type nonsense because it *is* nonsense, don't they? On the contrary: their delight (and ours too) comes from the fact that syntax does outstrip reality and that their conceptual structure is still (mercifully) fluid. But throughout, we (and they) remain firmly within the bounds of sense.

<sup>191</sup>Here I echo again Wright's (1981: 52) question. My thesis is a long (possibly overlong) reflection on that query. In a nutshell: I take this chapter to have given reasons to endorse—via other means—Pietroski's (2003: 245) (and the Chomskyan's) view that we can retain structural aspects to our theories while throwing out the idea that *they* explain (and provide support to) the meaning/truth-conditions connection. What worries me however is something the Chomskyan seems relatively uninterested in: how can we give an account of competence on which our language use comes out as fully *rational* once that connection is renounced?

<sup>192</sup>The point has been made by e.g. Dummett (1991: 13) and more forcefully still in his (1989: 172-73). Partee (1988: 49) argued that rather than CC, what PoC explains is indefinitely many semantic facts. Perhaps this is one of them. I am less sure about the oft-made claim (e.g. Janssen 1997: 457) that PoC is not an empirical principle but a methodological one—a claim often wielded to assess the plausibility of a particular meaning-theory (e.g. Wright (2001a: 344) and Chrisman (2011) with regard to Brandom's assertibilism and metaethical expressivism respectively).

properly so called.

I am aware the size of the task is a forbidding one. What I will argue though is that I can go some way towards bridging the gap between these two commitments of mine by proposing that our conception of content (both mental and semantic) ought to be revised downwards, as it were.

That is, I agree with Borg *et al.* that semantic content is minimal (and so is semantics). But I think content is a lot more minimal than even they think. It is radically, ineliminably minimal, that is, and consequently the compositional machinery operates on radically minimal pieces of content.

It would however be a mistake to think that semantic content minimally construed is either amenable to completion (it is not: it remains firmly minimal throughout; it is not a skeleton awaiting fleshing out) or to pragmatic enrichment (I don't think it pragmatically expresses indefinitely many propositions at all) or that it is *propositionally* minimal. And it would therefore be just as wrong to continue to insist on MDP (at least *as long as meaning is conceived in customarily robust terms*) and UaKTC.

I am thus fully with Chomsky (1975b: 43) when he says that the language faculty (including the compositional module) outputs abstract structures. I just happen to think that those structures are a lot more abstract than anyone thinks and that they *remain* wholly abstract throughout (semantic content is not *incomplete*: it is as complete as *content* can ever get).

Clearly, I am running wildly ahead of myself here. All that I wanted to accomplish in this chapter was breaking off the CC/PoC connection and sow the seeds for the idea that content *must* be radically minimal (i.e. radically disconnected from the determination of truth-conditions). With any luck, I have done enough to convince you of *that* (or at least to give you some pause for thought).

There remains a rather large puzzle about what rationally constrains interpretation.<sup>193</sup> I take up the challenge fully in the last chapter. By way of anticipation, my position is (initially, at least) fairly standard: we are rational to the extent that we are sensitive to reasons of the appropriate kind, given a specific field of enquiry.

The semanticist answer to the question regarding linguistic com-

<sup>193</sup>Explaining why some judgements as to grammaticality and meaningfulness are less inexplicable than others was, more or less, the challenge thrown by Evans (1981a: 341-2) to the (Wittgensteinian) opponents of systematicity (the fanatics wrecking the machines, as he colourfully put it on p. 326). That a theory of meaning should make sense of language mastery as a rational activity has also been argued for by e.g. Dummett (1987: 260) and (1991: 91).

petence was that we ought to be sensitive to the reasons provided by the atomic meanings. The complex meanings inherit *exactly* those reasons, and our competence is merely a question of *tracking* their transmission from below (via the compositional operations), with no additional work required.

By contrast, I shall end up arguing that we are rational language users to the extent that we are sensitive *to a much wider class of reasons*. We thus require a rationality that is ineradicably *situated*, one that is sensitive to situated reasons.

The task in this chapter was to try and show what compositionality could *not* be (i.e. the *sole* guarantor of the objectivity of meaning). In the next chapter my task is showing what *semantic content* could not be.

## Chapter 3

# The Roots of Sense

## How Deep Polysemy?

### 3.1. Introduction

In the previous chapter, I have considered (and given plenty of latitude to) reasons to distrust the idea that complex meanings are compositionally determined *solely* on the basis of constituent meanings and their mode of syntactic combination.

If sound, such reasons do appear to throw the entire semanticist enterprise into doubt, since they appear to force a considerable theoretical downgrading for the role of the compositional engine at its heart: its deliverances, I have argued, are *essentially provisional* in character (they await validation from speakers' judgements before complex meanings can properly be said to be compositionally determined).

A crucial part of my argument relied on the centrality of the notion of understanding to a proper conception of semantics (and indeed nonsense caused whatever trouble it did for the semanticist precisely because of *her* insistence on the CC/PoC explanatory connection).

In this chapter, I want to examine the question of what we should say about our understanding of atomic meanings, that is, our understanding of the base clauses that get fed to the compositional operations (remember: the semanticist needs a bottom-up version of PoC; without a base, there ain't going to be anything for the operators to operate upon).<sup>1</sup>

<sup>1</sup>Janssen (1997: 441) is refreshingly explicit about this. A central question here is what the aims of semantics ought to be. Some, like Davidson (1967: 32-3), have insisted that semantics does not have to say anything at all regarding (our grasp of) satisfaction conditions for predicates—linguistic semantics will leave that to conceptual semantics and take for granted that we have a story to tell about our apprehension of the base. Some neo-Davidsonians have embraced this (anti-“metaphysical”) stance—see the (brilliantly Fodorian) chapter 11 of Cappelen and Lep-



So, here's the plan for the chapter.

I start by reviewing, in section 2, the Meaning Determination Principle. In section 3, I move on to examining the semanticist's view of meaning for atomic sentences, with particular regard to the Content Expansion Thesis (the thesis that any effect of context can be *semanticised* by appropriate expansion of context-sensitive sentences).

Section 4 is devoted to the pragmaticist's criticism of that view; the claim to be assessed is that the disambiguating/de-contextualising process could never terminate (I also pose yet another dilemma for the semanticist).

In section 5 I turn on the heat for the pragmaticist by sketching a (more vicious) version of Grice's Circle in the form of a dilemma.<sup>2</sup> I argue that my dilemma shows the pragmaticist position to be (at least) as incoherent as the semanticist one.

My bleak conclusion is that on neither account does meaning individuation come out as a *rational* activity. There is a basic difficulty with securing determinacy of content, a difficulty that, whenever it has been raised, has routinely been swept aside—once again on (misguided) transcendental grounds—by a familiar *modus tollens* move (without determinacy, we could not exchange content; but we can, so content is determinate).<sup>3</sup>

I reject this move and I argue that we should instead take a hard look at the proverbial elephant in the room and revise our favoured accounts of rationality and normativity to accommodate a different notion of content (I discuss these questions in greater depth in chapter 6).

### 3.2. Meaning Determination and Content

Semanticist theories are committed to the thesis that the semantic content of a sentence is fully determinate (*that* is what distinguishes their view from the pragmaticist insistence on semantic underdetermination).<sup>4</sup>

ore (2005a). I think I have said enough in the previous two chapters to indicate my dissatisfaction with a position of this kind. Other neo-Davidsonians have been less demure. In e.g. Larson and Segal (1995: 127) we are told that predicates are associated “with general conditions of application”. Accordingly, the rules given in the semantic clauses are “rules for *determining* when a given word can be applied to a given thing” (ibid.: 126—my emphasis).

<sup>2</sup>Levinson (2000: 186; see also p. 399, fn.16).

<sup>3</sup>We have already seen this move mobilised in defence of PoC. In chapter 5, we'll see it again in defence of the standard conception of logic as the science of reasoning.

<sup>4</sup>There is a good account of the distance between the two views in Carston (2002: ch. 1)

The determinacy claim is intended to apply to sentences that are *closed* in a very strong sense: all possible assignments of variables (overt and covert) have been taken care of; all disambiguation has been carried out.

There is an old quote from Carnap that might help us setting up the stall for the semanticist:

By a **semantical system** (or interpreted system) we understand a system of rules, formulated in a metalanguage and referring to an object language, of such a kind that the rules determine a **truth-condition** for every sentence of the object language, i.e. a sufficient and necessary condition for its truth. In this way the sentences are *interpreted* by the rules, i.e. made understandable, because to understand a sentence, to know what is asserted by it, is the same as to know under what conditions it would be true. To formulate it in still another way: the rules determine the *meaning* or *sense* of the sentences. Truth and falsity are called the **truth-values** of sentences. To know the truth-condition of a sentence is (in most cases) much less than to know its truth-value, but it is the necessary starting point for finding its truth-value.<sup>5</sup>

This remarkably crystalline (and exhaustive) quote is from 1942. It nicely encapsulates a conception of meaning that is still fully operative in most semanticist frameworks today—and one that is certainly mandated within any broadly neo-Fregean account of content.<sup>6</sup>

Note the remark at the end: knowing the truth-condition for a sentence means knowing the *starting point* for its evaluation.<sup>7</sup> And the first question before us is: how *thin* (narrow) can that point be for the semantics to count as a faithful mirror of our competence?—how much information do we need to pack into it, how much representational power do we need to posit for our sentences in order for the MDP to be adhered to (and for it to be *workable*)?

Broadly speaking, the question is the one I already considered in the previous chapter, the one asking whether (or the extent to which)

<sup>5</sup>Carnap (1942: §7). There is a lot going on in this quotation; one thing worth noting is the straightforward equation being made between understanding a sentence, knowing the content asserted by its means *and* knowing its truth conditions which neatly exemplifies the EPU and UaKTC view.

<sup>6</sup>Of the kind defended in e.g. Peacocke (1986, 1992, 2008).

<sup>7</sup>There may appear to be lots of wriggle-room here. On the other side of the divide, Strawson (1950: 7) had after all said that “to give the meaning of a sentence is to give *general directions* for its use in making true or false assertions”. More recently, Neale (1999), Bach (1999a), Blakemore (2002: 30) and Dancy (2004: 196) have said similar things from very different perspectives. The point is however that the semanticist project is *defined* by its commitment to *strict* versions of MDP.

we can carve away semantic and worldly knowledge when we try to model our competence; whether, that is, we can *properly* model linguistic competence without ending up giving a theory of everything.<sup>8</sup>

Consider the case of vagueness: to be competent with colour terms is no more and no less than to know certain things about their behaviour (their conditions of applicability): for instance, that they are e.g. *tolerant*, in Wright's (1976) sense. But that means knowing certain things about the *world*, namely, that tolerance (a *semantic* property) is *forced* on us by precise limits on our ability for colour discrimination (a *worldly* condition)—*that's why* semantic competence requires us to treat those predicates as tolerant.

If so, and if vagueness is as widespread as philosophers since at least Russell (1923) have thought, there seems to be little chance of drawing a clear line between worldly and semantic knowledge (when I learn that 'red' is tolerant, I learn facts both about its semantics *and* the world—that's what a colour *is*, namely, something denoted by tolerant terms).

So, the question really is: how minimal, that is, how *non-wordly* can content be for it to carry out the job of *determining*, in a sufficiently robust sense, workable applicability conditions?

Generally speaking, expressions have epistemological and cognitive properties—or else they would not play the *normative* role that they do in our lives (semantic properties provide *reasons* for holding certain beliefs and acting in certain ways).

And for a theory to be both systematic and wide-reaching, the connection between these properties and the strictly semantic ones must be *functionally* determined.

Indeed, I'd argue that expressions carry epistemological and cognitive valence in virtue of their possessing *semantic* properties: it is *qua* bearers of content that they also have those "additional" properties.<sup>9</sup>

<sup>8</sup>As Chomsky (2000: ch. 2) famously complained against the opponents of I-linguistics. See also Neale (2005: 166, 178–79, fn.25) and Borg (2004: 80). Williamson (2007: 85ff.) attacks standard views of analyticity by giving cases where deviant speakers entertain wild beliefs about the world while still retaining linguistic competence—the cases resemble the ones discussed in Putnam (1962). The very intelligibility of these cases seems to suggest I'm wrong in insisting that we cannot partition worldly and linguistic knowledge with impunity. I reply that these cases do not concern matters of *how* sense *determines* reference, but only eccentric beliefs about reference. It is the semanticist, not I, who is committed to explaining meaning in terms of reference-determination. And my contention is: whenever ignorance of worldly matters affects reference-determination, then it *eo ipso* affects linguistic competence construed *à la* semanticist. In fact, the plausibility of Williamson's examples of *competent deviancy*, as it were, is evidence that I am right in arguing for the radical indeterminacy of our meaning stipulations (by the lights of semanticism, we are *one and all* competently deviant).

<sup>9</sup>This would need finessing, but my claim, roughly, is that epistemic/cognitive and semantic

In identifying the semantic content of an expression *as the content it is*, that is, I thereby apprehend that expression's position in the network of epistemological and cognitive nodes that structure my intellection of content.<sup>10</sup> To echo Sellars' famous turn of phrase: we place expressions in the space of reasons in virtue of their content.

Hence, getting clear about the *precise* character of that content goes hand in hand with becoming clearer about how best to conceive of the space of reasons itself (and thus of our rationality).

Accordingly, the burning questions in this chapter are: how determinate *can* semantic content be (can intensions *really* do the extension-fixing job that the Fregean requires?); and how *thick* can content get before it becomes computationally unmanageable?

In short, our task is to establish *the correct computational profile* for semantic content (neither too thin, nor too thick) so that our linguistic practice may count as genuinely rational—on the semanticist story, we are rational to the extent that we are competent *rule-followers* with respect to the dictates of the meaning functions individuated by our best linguistic theory.

I do not think there is any question that semanticists are (and must be) committed to content determinacy (and MDP).<sup>11</sup> Nevertheless, I think it is useful to try and motivate this commitment a little more.

Consider what is involved in the idea that sense provides a *cognitive fix* on reference.<sup>12</sup> Fregean thoughts are senses of declarative sentences, but their expression is not separable from the (attempted) *identification of a referent* (more generally: its contribution under embedment to that general task).<sup>13</sup> And so, *it is part of the thought*

properties are strongly supervenient (in the very strong sense of e.g. Kim 2003: 561). We could not have *schmpeakers* who are sensitive to one class of properties but not to the other. It was Frege, after all, who defined sense as the *cognitive* value of expressions.

<sup>10</sup>This is why establishing whether a rule-based account of language is viable has implications for the philosophy of mind. The truth of connectionist accounts of mind goes hand-in-hand with a corresponding theory of language. Dąbrowska (2004: ch. 8) has a useful sketch of the debate in this area. Dawson (2005) is a good introduction to connectionism.

<sup>11</sup>See again the references in fn. 28, section 2.2. I hope that my discussion there has provided enough grounds to distrust one familiar line of resistance, namely, the one that claims that truth-conditional semantics is merely concerned with establishing systematic connections among assignments of semantic values. Additionally, without determinacy of *sense* it is not even clear how we can separate logical from non-logical vocabulary. I return to this question in chapter 5.

<sup>12</sup>I borrow this happy label from Wettstein (2004: 13).

<sup>13</sup>If you don't like *that* idea, think of this as: its contribution under embedment to the determination of which proposition is associated with the sentence (and modify the chapter's argument accordingly; if it is any good, it will still go through). In any case, it is a standard *semanticist* assumption that what matters about a sentence is *how it gets to be true (or false)* in virtue of its structure (see e.g. Davidson 1969: 49). The worry about semantic underdeterminacy is that no sentence can

*being what it is* that it (attempt to) identify a certain referent (in a certain way).

The (purported) act of identifying the referent *in a certain manner* is what makes a thought *the* thought it *is* (or else we'd have a code and not a language).<sup>14</sup> If a thought does not succeed in that referent-determining task (if there isn't *enough* to its content to achieve that task) then it is not *that* thought.

What we want in our semantic clauses, then, is a way of spelling out the content of an expression in such a way that all ambiguities are removed (there must be no uncertainty as to *which* thought a sentence expresses, which semantic contribution an expression makes under embedment).

The semanticist's insistence on systematicity thus requires that linguistic theory give a proper account of determinacy of content for the atomic clauses and a demonstration that the compositional operations are determinacy-preserving.<sup>15</sup>

In short, what the semanticist needs to show is that it is possible to tame the (pretty general) semantic underdetermination of content—the fact, that is, that economy of conversation (of information processing, if you wish) has dictated that languages commonly employ highly compressed (and therefore highly ambiguous) surface forms.<sup>16</sup>

Clearly, ineradicable ambiguity and underdetermination (or underspecification) could not be countenanced by the semanticist because of her commitment to the thesis that linguistic rules *determine* the proposition expressed by an utterance of a sentence.<sup>17</sup>

succeed in determining what it takes for it to be true (or false).

<sup>14</sup>It is not coincidental that compositionality and content determinacy are both essential to something counting as a language rather than as a code. I grant both requirements. What I am trying to do in this thesis is showing that we need to revise our conception of both notions to respect the data about language mastery.

<sup>15</sup>The semanticist may or may not insist that the cognitive content of expressions be *fully* transparent to the language user. In either case, the assumption will be that *something* ensures determinacy of content. I agree with McDowell (2005: 168-69) that the Fregean notion of sense is best explained in terms of preserving ascriptions of rationality to speakers caught in Frege's puzzle situations. Accordingly, *some* degree of content transparency is required.

<sup>16</sup>I am not endorsing the 'therefore' here.

<sup>17</sup>The notion of *what is said* by the utterance of a sentence-token-in-context is a highly controversial one. Levinson (2000: 195) has a helpful table of the various views on this issue. See also Recanati (1989, 2001) and Bach (1999b, 2001, 2004). For Levinson (2000: 186-7), what is "said" is (linguistic) truth-conditional content, the implicated is pragmatic inference. Carston (2002: 96) and Recanati deny this (for different reasons!). See also the recent discussion in Recanati (2010: 12-14). I won't enter the controversy here. All I am interested in is purely semantic content, the first level of *seemingly* truth-evaluable content that we can reach. In any case, the argument I am

The semanticist therefore requires a bridge between ambiguous surface form and unambiguous logical form. My argument in this chapter is directed against the very idea that such a bridge could ever be built.<sup>18</sup>

Indeed, the main burden of this chapter is to remind the semanticist (*and* the pragmaticist!) of an issue that is all too often swept under the carpet: the threat posed by radical indeterminacy of content to *any* account of linguistic competence as an eminently *rational* capacity.

### 3.2.1. Individuating Content

How do we individuate content anyway? How can we tell *what* a sentence says?

There seem to be two broad answers to this last question: proof-theoretically, as it were, i.e. by saying how we would *use* it (two sentences say the same thing if they are indistinguishable in use; they allow moves from the same grounds to the same conclusions);<sup>19</sup> or model-theoretically, i.e. by saying that the sentence makes a representational claim and that what it says is that things are as it says they are (the character-level intensional rules determine the extensional facts described by the sentence's content).<sup>20</sup>

I'll assume semanticism is relying on a model-theoretic answer to the question. So here's the problem faced by the semanticist: if the semantic rules are to determine semantic content, then there'd better not be two sentence-tokens such that, all things deemed relevant by the semanticist being equal, they nonetheless differ in truth-value.

In broadly Kaplanian terminology: if there is no difference in (what the semanticist says we should put in) the (entities assigned to the) indices and in the circumstances of evaluation and yet there is a difference in truth-value (and hence in content), then there *must* be a difference in character (we hadn't individuated the logical form of the sentence finely enough and the index is incomplete).<sup>21</sup>

rehearsing is directed against the possibility of *ever* saying *anything* definite. If sound, it will affect *any* conception of what is said and should not fall prey to Salmon's (1991) pragmatic fallacy.

<sup>18</sup> Actually, it is directed against the idea that there *is* such a thing as unambiguous logical form.

<sup>19</sup> On this approach, we would be generalising to all expressions the inferentialist account of the meaning of the logical constants given in the proof-theoretical tradition of Dummett and Prawitz.

<sup>20</sup> There are other options available of course (e.g. context change potential etc.) but I think I can stick with the classic, extensional model-theoretic conception of same-saying without loss of generality.

<sup>21</sup> Actually, the terminology I have in mind is Predelli's (2004: 14, fn. 2)—minus the choice of spelling: indices, then, contain everything needed to fix content-in-context so that the latter can then be evaluated.

This is what it means for (character-level) rules to determine content. So for the semanticist any variation in content must be traced back to a rule-governed variation at character level.<sup>22</sup>

The paradigm case is of course sentences containing indexicals: utterances of e.g. ‘I am hungry’ will express different contents (and have different values) at different points of evaluation. The semanticist will insist that all contextual influence on content can be dealt with in a similar rule-governed fashion (that’s what MDP states).

### 3.2.2. Arguments from Indeterminacy

The line of attack against this view usually involves some manifestation of the *Travis effect*,<sup>23</sup> the apparently indefinite diachronic variation in truth-value for sentences involving sublunary predicates, which is meant to establish their essential occasion-sensitivity (it is our interests and parochial judgements that determine the truth value of a sentence predicating, say, roundness of a given ball, not the semantics).<sup>24</sup>

I take it as given that the Travis effect poses a threat to semanticism. In this section, I want to argue that the need to implement MDP and thus keep indeterminacy (and Travis) under control commits the semanticist to a *Content Expansion Thesis* (CET) which I’ll make precise shortly.

The problem faced by the semanticist actually takes two forms: a weaker one, which we can call the thesis of the Underdetermination of Semantic Content (USC);<sup>25</sup> and a stronger one, which we can call the Radical Pragmaticist Argument (RPA)—it’s stronger because it is an argument for the *radical* underdeterminacy of *any* kind of content.

USC claims that, for a sentence *S*, linguistic rules only determine *S*’s *linguistic* meaning: in (almost all cases) they do not reach far enough

<sup>22</sup>I’m being a bit sloppy here, but I hope it’s clear what I’m up to. The point is that the character level rules will return the same values for the same arguments. So if two sentences intuitively differ in truth value while the character level rules *and* the entities assigned to the indices are the same, then we must have overlooked some elements that should have been included in the indices. For any variation in truth value must be traced back to the stuff in the indices.

<sup>23</sup>Sainsbury (2001: §4.5).

<sup>24</sup>See the essays in Travis (2008).

<sup>25</sup>This is very rough as it stands. Carston (2002: 19) usefully distinguishes three shapes the thesis can take. Two are unproblematic and uncontroversial: 1) linguistic meaning underdetermines what is meant (i.e. *communicated* content is underdetermined by linguistic content); 2) what is said underdetermines what is meant. The third one is the bone of contention between semanticists and pragmaticists: linguistic meaning underdetermines what is said (the proposition expressed). See Neale (2005: 193) for another statement of the thesis.

to determine *the* proposition intuitively expressed by an utterance of *S* in a given context—the slogan: linguistic content is, properly speaking, *propositionally incomplete*.<sup>26</sup>

In many cases, adherents to USC seem content with the claim that we should replace truth-conditional semantics with truth-conditional pragmatics—while still leaving open the possibility of giving a (largely systematic) *theory* of meaning.<sup>27</sup>

All that needs doing is adding a story about *pragmatic competence* to integrate the traditional one about semantic competence: the mistake of the traditional paradigm was in thinking that we could (and should) keep the two separate.<sup>28</sup>

The demand then is for a *realignment* of the theoretical and explanatory priorities: semantics is not the (modularly constrained) prelude to pragmatics,<sup>29</sup> but rather the two are joint partners—and in fact, pragmatics can be autonomous of semantics in setting certain parameters not mandated (or indeed marked for) at the level of linguistic content.<sup>30</sup>

In any case, on this view we *do* reach a level of propositionally complete content; the view differs from the semanticist's in insisting that without pragmatic intervention *right into the fabric of linguistic content* we could have no truth-evaluable content at all.

By contrast, the RPA does not just endorse USC and the consequent request for theoretical reform: it adds the further claim, not strictly

<sup>26</sup>The USC thesis in and of itself leaves it open whether the gap between linguistic meaning and proposition expressed may be bridged by a wider theory of linguistic communication that makes *essential* appeal to pragmatic processes.

<sup>27</sup>See e.g. Recanati (1993: ch. 13), Bezuidenhout (2002), Kadmon (2001).

<sup>28</sup>See Recanati (2010) for a book-length defence of this view. Recanati's position is an intriguing mix of elements from the two sides of the debate, for he seems to join a belief in the theoretical tractability of contextual factors (e.g. p. 9) with a view that content may be radically underdetermined (he also seems happy with the contrast literal/metaphorical meaning, see p. 4). Note that Recanati's defence of truth-conditional pragmatics (TCP) makes him vulnerable to the arguments for indeterminacy that I give in the text, for TCP still espouses a version of the MDP. According to Recanati (2004a: 134–35), compositional operations process not just semantic information but also what he calls *modulated sense*, that is, pragmatically enriched semantic content. Recanati is insistent though that his framework is still grammar-driven, for he wants to resist accusations that TCP is unsystematic. To an extent, TCP could be seen as an implementation of a view of compositionality as a *semi-productive* process. While sense-modulation is unsystematic, sense-composition still retains systematicity. I find this a curious failure of nerve.

<sup>29</sup>Montague (1970c: 223, fn. 2) famously dismissed syntax as little more than a preliminary to semantics.

<sup>30</sup>This is the distinctive claim of Recanati (2004a, 2010). The distinction is between saturation (roughly, the filling in of the slots in the index) and free enrichment (there is, the pragmatist claims, no slot at Logical Form (LF) corresponding to the pragmatically-controlled content-completion procedure).



entailed by USC, that *no* semantic theory of the kind envisaged by semanticists and formal pragmaticists alike can explain (and/or accurately predict) the variations in truth value above because pragmatic effects on content cannot be captured in full generality by *any* theory—they are ineradicably interest-relative and occasion-sensitive.<sup>31</sup>

More seriously still: there can be no notion of *stable* content. Our cognitive lives, that is, are immersed in indeterminacy and the semanticist project can cover only a very limited part of our cognitive horizon.

The challenge for the semanticist (and the pragmaticist of formal leanings), then, is how to secure determinacy of content against USC and RPA.

### 3.2.3. Semanticist Responses

Typically, the semanticist will respond to the challenge in either of three ways: *a*) treating NL sentences as shorthand for fully specified sentences (what were once called ‘eternal sentences’);<sup>32</sup> *b*) going indexicalist so as to absorb as many contextual elements as possible into *semantic* content—truth-conditions are made determinate by linguistic content, the idea goes, via the incorporation into the LF level of appropriate tuples of variables that get assigned a value at a context.<sup>33</sup>

In both cases, we preserve a homomorphic mapping from syntax to semantics either by associating a given ‘occasion’ sentence with a range of eternal ones, or by taking sentences to be something akin to (pretty gigantic) Russellian propositional functions made ‘complete’ by the absorption of relevantly salient contextual factors, with the process of absorption firmly (and crucially) under the control of linguistic entities (either overt or covert, that is, tucked away at the LF level).<sup>34</sup>

Note that there is a sense in which the two strategies coincide,<sup>35</sup>

<sup>31</sup>Travis is the main writer I have in mind, along with e.g. Dancy (2004).

<sup>32</sup>I.e. sentences where all ambiguities are resolved, all indexicals assigned a reference, and so on.

<sup>33</sup>The indexicalist label originated with Recanati (2004a), In his (2010), however, Recanati has now adopted the label ‘minimalist’ for a position like Stanley’s (2007). I prefer to keep the latter label for positions such as Soames (2002, 2005a), Borg (2004) and Cappelen and Lepore (2005a).

<sup>34</sup>A weaker variant of this view has it that sentences express *skeletal* propositions (partial thought contents) that await pragmatic infills (there is no *need* to posit hidden indexicality), see e.g. Bach (1987/1994: 76-7).

<sup>35</sup>With one important distinction: eternalist-theorists will typically be committed to the complete elimination of genuinely indexical expressions in the non-relativised translations of occasion-sentences. This raises the problems discussed in e.g. Dummett (1973: 384), Perry (1977, 1979), Evans (1981b) and Dummett (1981).

in that they both posit a deep level of semantic disambiguation that is supposed to parry the problem raised by the RPA challenge (they both want to *semanticise* context, absorbing into semantics as much of it as is needed to fix content).

Neither approach tells us *what* does the disambiguating on a particular occasion of use, but the central claim common to both responses is that (the satisfaction of) any intuitive truth-condition associated with a specific utterance is linguistically controlled (in context, we say big things by deceptively small means, that is).

The third and final option open to the semanticist is *c*) to go minimalist, whittling semantic content down to anodyne proportions: semantic content is precisely as skeletal (and ‘ambiguous’) as it appears to be—on this account the multiplicity of meanings associated with an expression is accounted for by speech-act theoretic considerations or by a broader, more liberal conception of the notion of truth-conditions (as we discussed in the previous chapter).<sup>36</sup>

Clearly, the first two options keep semantic content thick—what we *really* say has a rich, thick texture; on the first view, reasons of conversational economy dictate that we use the shorthand version, while on the second, it is an essential feature of language that syntactically controlled hidden variables get assigned values in context while the surface structure remains lean and conversationally manageable.

The third version, however, thins down the notion of semantic content to proportions that some may find uncomfortable. In this chapter, I’ll concentrate on the first two views. I return to minimalism in chapter 6.

<sup>36</sup>See the already cited Soames (2002, 2005a), Borg (2004), Cappelen and Lepore (2005a). I take the indexicalist/minimalist contrast from Recanati (2004a: §1.2; §6.2). Unfortunately, as I have said, Recanati (2010: 5, fn. 3) has somewhat muddled the waters. The contrast I want to draw is between positions that remain pretty much disquotational (*this* is what I call minimalism) and indexicalist positions that either absorb context into content directly and thus make contextual influence a *fully semanticised* affair (Stanley) or split content across sentence-index pairs (Predelli). Recanati (2010: 14, f.11) now call these positions I-Minimalist (semantic content coincides with the intuitive truth-conditions we assign to sentences without positing top-down pragmatic intrusion). Recanati (2007a) can be taken to be a variety of indexicalism in this second sense; it differs, however, in the way in which the determination of the indices is secured. It is also interesting to note that a position like Travis’ (2006a: 151) is very close to the minimalist one. The divergence is over whether *truth* can properly be predicated of sentences. On Travis’ shocking view of semantics, semantics is something without a truth definition—and the gap is not filled by giving a proof-theoretic account of applicability conditions either, at least not in a conventional sense (see Travis’ (2000: 212ff.) notion of introduction/elimination *policies*).

### 3.2.4. The Flight from Context

In spelling out the CET, I find it useful to go back to the *Begriffsschrift*, where Frege drew a contrast between formal languages and NL that centred on the notion of *guesswork*. The entire purpose of his concept-script was to leave *nothing* to guesswork.<sup>37</sup>

A formal language, that is, is a language such that logical forms can be read off its expressions *directly* (UaGS in its purest form, that is). Fregean thoughts, the invisible objects *captured* by the language of *Begriffsschrift*, are the senses of context-free sentences, sentences free of all indexicality.<sup>38</sup> In contrast, natural language sentences “leave a good deal to guesswork”<sup>39</sup>—there is some (a lot of) work to be done to recover the form underlying surface structure.

We could thus take CET to be the thesis that we *can* eliminate guesswork from NL semantics, that we can fill the gap between what surface structure seems to express and its context-free content-specification.

For Frege, banishing guesswork means implementing the requirement of *Eindeutigkeit*: signs must be made to bear their meanings univocally, i.e. they *can only mean one thing and one thing only*, just like thoughts are supposed to.

In his all-too-brief discussion of the all-pervading role of guesswork in NL, Frege offered remarks about contextuality that are, despite their brevity, subtle and far reaching. Because of their terseness, they are open to two diverging interpretations.

Generally speaking, the need for guesswork arises from the fact that (most) NL sentences are incomplete in the sense that they do not express a complete (i.e. truth-evaluable) thought. The semantic content of such sentences is, as we noted, underdetermined and requires completion via “accompanying circumstances.”<sup>40</sup>

We could then read Frege as suggesting that we can treat the incomplete linguistic expression as *naming* “a single determinate object” by *including the circumstances of utterance into semantic content* via appropriate enrichment/completion (Frege speaks of *Ergänzung*).

On a different reading, we could instead think that what Frege had in mind was that the accompanying circumstances are (literally) part of *the act of expressing/grasping* the thought; circumstances, however,

<sup>37</sup>“Nichts wird dem Errathen überlassen”, §3. See also Frege (1880/81: 12-3), (1882: 72/51) and (1915: 252).

<sup>38</sup>As Carruthers (1984: 1) noted.

<sup>39</sup>Frege (1914: 213).

<sup>40</sup>Frege (1914: 213). See also Frege (1923: 34, fn. 8).

are not incorporated directly into semantic content.

Semantic content, that is, remains incomplete and is enriched only metaphorically, as it were. Content, on this second interpretation, is enriched via *situatedness* (we *take in* gestures and circumstances and *thereby* secure determinacy), but the circumstances remain *external* to semantics properly conceived.

This, I think, is a rather radical reading of Frege as a rather unlikely *ur*-pragmaticist. I'll return later to this second conception of the content/context interaction (it's the one that in fact I end up endorsing). In this chapter I want to concentrate on the first option and a less radical version of the second.

### 3.3. The Content Expansion Thesis

As I said, I am going to consider two ways of absorbing contextual influences into semantic content, the eternalist and the indexicalist strategy.

Both strategies can be subsumed under CET, which we are now ready to state in full:

**Content Expansion Thesis:** Any NL sentence containing underspecified expressions can be expanded to a maximally specific sentence.<sup>41</sup>

CET looks innocuous enough, but is in fact a highly substantive thesis. It asserts that we *can* regain *Eindeutigkeit* for our vernacular, that our sentences can be made to represent univocally and in a maximally

<sup>41</sup>Where a sentence is maximally specific if none of its constituents is underspecified (adding further words would not close off any interpretive nodes)—we could also say it is *maximally regimented*. Under 'underspecified' I'm including here expressions that are polysemic, ambiguous or indexical in character. A thesis of this kind has been discussed in the literature under different labels. Recanati (2007b: 119-120) calls it the *Transformability Thesis*. A related cluster of theses is that of various *Effability Principles*, stated in e.g. Katz and Searle. See Sperber and Wilson (1986/1995: 191-2) and Carston (2002: ch. 1) for discussion. One of the earliest statements of the thesis is in Carnap (1937: 168). Williamson (1994: 51) has an instructive discussion of Peirce's conception of determinacy as absolute specificity of content. Stanley's (2000: 32) claims that syntax assigns to each NL expression *token* a "lexically and [...] structurally disambiguated structure" while also arguing that such structures do *determine* content. Chomsky (1955-56/1975: 309) had also insisted that the end result of transformations must be unambiguous and as recently as in Boeckx (2008a: 244ff.) we find the (extraordinary) claim that ambiguity is something the language faculty "seeks to avoid". Syntax (and semantics) is thus the *elimination* (or overcoming) of ambiguity, a conception of language that as Derrida (1982: 247-48) has noted goes back all the way to Aristotle, *Metaphysics*, IV, 1006<sup>a</sup>34-1006<sup>b</sup>13. It is also to be found in Frege (1906: 303): "[a] word without a determinate meaning has no meaning".

specific manner.<sup>42</sup>

It is, that is, a claim about content: semantic content can be *eternalised* (either directly or by indirect mapping), it can be made absolute, *entirely* independent of circumstances (thus latching on to the corresponding determinate content in the sphere of concepts).

### 3.3.1. Implementing CET

CET seems to incorporate the following two claims (the first is a special case of the second, really): that indexicality is eliminable without residue (i.e. without loss of information, as Bar-Hillel 1954: 366 put it); that any (context-infected) sentence can be expanded until its content is fully and exhaustively specified.

Both claims, as it turns out, are deeply problematic. We should also note that it seems natural to modify CET by adding the clause: ‘bearing the same meaning’—indeed, that seems to be *the point* of the thesis. We can, that is, read CET as stating a *meaning equivalence* thesis between the original sentence and its maximally specific expansion.

As we shall see, this causes some awkwardness, for it hardly seems plausible that the two sentences will be cognitively equivalent. At best, the thesis is about extensional equivalence only. But there are reasons to doubt even that.

I said above that there were two ways of mapping NL sentences to their fully determinate counterparts. Let me take them in turn now.

#### 3.3.1.1. CET-Eternalism

Arguably the most influential statement of the post-Fregean eternalist strategy is in Quine (1960: 193):

to specify a proposition without dependence on circumstances of utterance, we put for the ‘p’ or ‘[p]’ an *eternal* sentence: a sentence whose truth value stays fixed through time and from speaker to speaker.<sup>43</sup>

<sup>42</sup> An idea mooted only to be derided by Wittgenstein (1945/1953: §91). The illusory goal being “a state of complete exactness.” One problem with that view is brought out in (§426): “A picture is conjured up which seems to fix the sense *unambiguously*. The actual use, compared with that suggested by the picture, seems like something muddled. Here again we get the same thing as in set theory: the form of expression we use seems to have been designed by a god, who knows what we cannot know.” Note that CET *presupposes* that signs *are* unstable: or else, how could we *shorten* eternal sentences for everyday use?

<sup>43</sup> See Frege (1893/1998: xvii) for its origins.

The CET-like eternalist claim is that for any NL sentence, there's an eternal sentence that is its equivalent in content and that, as Kaplan (1977: 506) put it, expresses the same proposition in all contexts.<sup>44</sup>

As Quine intended it, the notion of eternal sentence explicitly aimed to *abolish any distinction between type and token*—there would only be tokens of uniquely-instantiated eternal sentences.

Securing absoluteness of truth value for its sentences, then, is the main requirement for bringing NL nearer to the convenient docility of formal languages.

Clearly, a language containing only sentence tokens, one for each possible context of utterance, would be horrendously complex. The point of course is that by assuming CET we can *treat* sentence types (or rather: their tokens) *as if* they were unique tokenings of eternal sentences—given enough time and computational resources, it would always be possible to fully unpack their content.<sup>45</sup>

### 3.3.1.2. CET-Indexicalism

On the CET-Eternalism approach, we (more or less brutally) map ambiguous sentences to (allegedly) non-ambiguous ones; we thus stay *within* the object language (the fully eternalised sentences are part of that language).

Under CET-Indexicalism, we may endeavour to remove guesswork by two different strategies.

We could keep semantic content relatively slim and add indices that make explicit the content dependence *only in the semantic clauses* (thus transferring the disambiguating role to the metalanguage).

Or we could instead posit an array of variables at LF so that disambiguation is carried out still within the object language but, unlike with the eternalising strategy, the job is now being done covertly (as speakers, we are both insensitive and yet responsive to the *hidden inflections* that our utterances add to the sentence types: their morphology is invisible, but no less real for that).

If you like labels, we could call the strategies *Clause-Indexicalism* (the variables in search of an assignment are stored in the metalin-

<sup>44</sup>The claim is made in precisely these terms in e.g. Quine (1970: 13) and Soames (1999: 17). All that is required, Quine thought, was supplying “names and dates and cancelling the tenses of verbs.” An early statement of this position is in Frege (1904: 286).

<sup>45</sup>It is interesting to note here that Quine (1970: 14) was already aware of two features of eternal sentences that should have given pause: that even eternal sentences had a degree of relativity, in that they too had to be indexed to a language and a time; and that somehow *those* aspects of their relativity *could not* be expressed *in the language itself* but had to be “tacitly understood” as applying to the telescoped sentences, *even in the case of mathematical statements*.

guistic semantic clauses) and *LF-Indexicalism* (LFI) (the variables reside at LF and are part of the object language) respectively.<sup>46</sup>

Each of the three strategies, Eternalism and the two varieties of Indexicalism, will be vulnerable to the RPA at different places. But they all share a leading idea, namely, that the *maximal regimentation* of NL into a formally treatable language is achievable by some means or other—and that’s what will allow us to regain the semantic paradise where polysemy gets no footing.

And on this story it is CET that guarantees that we can make sense of the fact that we *do* succeed in communicating thoughts to each other (the pieces of common content we exchange in conversation are the fully determinate items posited under CET).<sup>47</sup>

As noted, many contemporary semanticists are seldom explicit about the CET, but I contend that they are all committed to some version of it and that there is no way of making sense of their projects (and claims) without attributing something like it to them—what else but CET could implement MDP?<sup>48</sup>

Reflect also that truth-conditional semantics relies on semantic content making a representational claim about reality that is at least *potentially* determinate (even Borg’s liberal notion of truth-conditions requires that each of the candidate conditions in the set be determinate enough to admit of satisfaction by *some particular* state of affairs).

For the semanticist project to succeed, then, polysemy must be tamed. CET and its variants bring that requirement into sharp relief. I commend the thesis to the practising semanticist.

<sup>46</sup>I borrow the ‘indexicalism’ label from Recanati (2004a). As I remarked in footnote 33, his terminology has somewhat shifted in his (2010).

<sup>47</sup>The “argument from communication” is often (if not regularly) invoked in support of one’s thesis by many semanticists, see e.g. Cappelen and Lepore (2005a, 2006, 2007). Russell (1919/1993: 195–96) rejects it altogether (for rather idiosyncratic reasons). There are more recent alternative proposals regarding ‘shared’ communication, see e.g. Gauker (2003) and Barker (2004).

<sup>48</sup>Goldfarb (1997: 87) brings out this commitment rather nicely. Kaplan (1977: 523) may appear to deny this when he says that rules determine content without being part of it. This is an impeccable remark, but it should not hide the fact that *a*) the rules themselves must be stated determinately (they are *one* of the objects of our knowledge of meaning); *b*) the content *determined* by those rules is itself determinate (it is *the* value of a function) and is *modelled* either by the semantic clauses or the hidden content at LF. I am here assuming that semantic rules are determinate iff they determine determinate content. Given the semanticist’s other commitments I cannot see how she could coherently deny this.

### 3.4. The Radical Pragmaticist Argument

So, what makes trouble for CET?

Usually, anti-semanticist arguments centre on *diachronic* contextual infiltration—causing grief to the semanticist requires a comparison *across* contexts, a pairing of contexts where two tokens of the same sentence-type intuitively receive different truth-values without a discernible difference in intension (or in the indices involved), or without a difference that could be captured by a systematic theory (that’s the Travis effect again).<sup>49</sup>

I’m actually interested in a much more fundamental (and much more insidious) problem for formal accounts of meaning (as I said: the problem affects truth-conditional pragmatism as well).

It is *synchronic* contextual infiltration that worries me. Our difficulty, that is, arises *already* within single-context cases, and it poses a headache for *any* theory of meaning.

#### 3.4.1. Another Halting Problem

The claim made by CET is that there is a process, namely, the telescoping of semantic content until all ambiguity is removed, that is deemed to be a terminating one. What could go wrong with this process?

It seems as if there are two bad things that could happen: the process never halts, and so we would never reach an extensionally equivalent maximally specific sentence (polysemy is ineliminable); or it does halt, but its output is neither extensionally, nor intensionally equivalent to the original sentence.

My claim, on behalf of the radical pragmaticist, is that there is actually no other outcome. These are the *only* two things that can happen. And both are bad. So CET provides no solace to the semanticist. Polysemy is here to stay.

In fact, I’ll conclude that the only choice for the semanticist is to stick with minimalist content (and adjust her claims and outlook on meaning, and perhaps life too, accordingly).

Let’s consider the argument for the non-terminability of the process first. Here’s another quote, I’m afraid again from Wittgenstein:

A sentence like “this chair is brown” seems to say something enormously complicated, for if we wanted to express this sentence in such a way that nobody could raise objections to it on grounds of ambiguity, it would have to be infinitely long.<sup>50</sup>

<sup>49</sup>See the recent arguments to this conclusion in Recanati (2010).

<sup>50</sup>Wittgenstein (1914-16/1998: 5); compare Frege (1906: 301). A fascinating anticipation of this



Let me stress right away that it is not just the presence of standard indexicality (the demonstrative ‘this’) that is the source of the problem here.

The trouble goes further than that and affects, as we shall see, expressions (such as sublunary predicates of the ‘*x* is brown’ kind) that are far less amenable to formal treatment than the familiar class of indexical expressions. In fact, the problem is fully general; it concerns the very nature of signs.<sup>51</sup>

The Wittgenstenian idea is disarmingly simple: signs point in indefinitely many directions. They require an interpretation to specify their meaning (and note that the worry here is to do with indeterminacy of *sense*, not Quinian indeterminacy of reference). Each occurrence of a sign generates indefinitely many meanings there for the taking. Any interpretation would be couched in a sign-like fashion, and would thus reproduce the problem at a higher level. Hence, determinacy cannot be secured by semantic means.<sup>52</sup>

Now, suppose the point is sound. Then CET has got a very basic design flaw, for it states that we can rid ourselves of the essential polysemy of sign *by adding more signs*.

And the objector will complain that it is obscure that by adding ever *more* words as the sentence gets expanded we would be able to close off *all* underdeterminacy. Would not each additional word introduce *more* underdeterminacy in turn?<sup>53</sup> After all, as Putnam would put

sort of argument is found in Reid (1764: Ch. 6 §24). Thanks to Katherine Hawley for indirectly drawing my attention to this proto-pragmaticist line. For more recent examples, see Searle (1978: 212), Travis (1985: 200, 207), (1997: 112–113), (2000: 91), (2006b: 28–9), (2006a: 40) and (2008: 2, 6), Wheeler (2000: 24), Recanati (2004a: 54, 58, 95) and Bezuidenhout (2002: 105, 108, 113).

<sup>51</sup>I am saying two things here. That there is a *specific* difficulty hiding behind the most mundane of predicates—it’s not clear e.g. how we could specify the *sense* of ‘brown’ in a way that would provide for *all* cases (as Zettel §440 put it, we can *always* construct doubtful cases where our stipulations regarding applicability would be silent). In short, sublunary predicates are deeply undecidable. Secondly, there is a *general* difficulty due to the nature of signs: signs, *qua* signs, can and do point in indefinitely many directions. Again, no stipulation can ensure univocality in a given case.

<sup>52</sup>There is more than a whiff of the Tortoise Regress from Carroll (1895) here. So one might think that I am saddling the semanticist with a view similar to the one Dummett (1973: 596) attributes to Quine (and criticises along anti-CET lines). The lesson seems to be that there must be rules of representation that cannot be part of a theory, that cannot be accessed in consciousness on pain of infinite regress. One could then protest that the semanticist is not committed to Quinian *theoretical* holism anyway. Sure. But as long as MDP is in place, a commitment to CET will also be in force.

<sup>53</sup>See Putnam (1970: 140–42), Ziff (1972: 131), Searle (1980: 228) and Beaney (1997: 32). Posner (1980: 186–87) raises the issue of the resurgence of ambiguity in the clauses used to precisify the original meaning. Slightly different but related worries are forcefully argued for in Gross (2001: ch. 3). Yet another anti-expansion argument is in Dancy (2004: 196–197). Horwich (1990/1998: 100) also declares himself impressed with the anti-CET RPA. Carston (2002: ch. 1) is again a good guide here.

it, at any further step in the expansion process we are simply adding just more *theory*, as we attempt to close off all available readings bar one.<sup>54</sup>

It seems then as if the semanticist, by conceding that there *is* a problem at the *first* stage (i.e. by admitting that NL sentences are infected with polysemy) has barred herself from a solution (her claim that we can do something about polysemy by regimenting NL is weakened by the fact that she is simply helping herself to more of the same stuff: signs).<sup>55</sup>

There is a temptation to say that the problem only affects the Quinian formulation, that the problem is internal to the object language. Granted, expanding the sentence in the object language won't secure determinacy of sense, but if we go the Clause-Indexicalist way instead and specify senses in our semantic clauses, formulated in a quasi-formal metalanguage where ambiguity is banned, then the problem cannot even arise.

Or can it? Why should we think that the *metalanguage*, whether formal, semi-formal or informal, should be exempt from what seems to be an *essential* mark of the sign, i.e. its inability to self-authenticate its interpretation?

Any formal language, by definition, will need *setting up*, and the stating of its vocabulary, signature and rules of inference will itself be a linguistic act carried out by linguistic means (that's why Brouwer was so insistent that mathematics is a purely *mental* act).<sup>56</sup>

Much as I wish it'd go away, I think the problem is stubbornly genuine, and genuinely general (it affects *any* language, whether natural or formal, ordinary or extra-ordinary).

#### 3.4.1.1. Rescuing CET

One could take two views regarding this stumbling block for semanticism.

We could think that it is in principle impossible to eternalise away context-dependence because of *our* inability to articulate clauses that can specify the precise content of context-dependent thoughts.

Or we could say that the limitative result is deeper, that even godly powers could not pin down a thought with the required amount of precision. We could think, that is, that the indeterminacy phenom-

<sup>54</sup>Putnam (1977: 18).

<sup>55</sup>A diagnosis of the rule-following issue along these lines is in Luntley (2003: 25). Although structurally similar, the two problems remain however distinct.

<sup>56</sup>van Dalen (2002: 2).

ena affect not just linguistic content, but content *tout court*, and thus intentionality as a whole.<sup>57</sup>

I happen to think the latter is the case and that this requires a revision of mainstream views of rationality. I'll take up these issues again in chapter 5 and 6. In the remainder of this chapter, however, I restrict my discussion to the case of semantic content and *our* ability to capture it theoretically.

We have seen that the RPA attack on the determinacy of meaning centres on the claim that meaning is inherently unstable. And if so, CET is false, because no *sentence* (no string of signs) could ever be eternal.

But what reason do we have for thinking that the expansion process would *never* terminate? We've not really been given a *proof* that it cannot be done, at best a sketch of a proof that we are supposed to accept as definitive.

Or better: the radical pragmaticist has thrown up a challenge. Fix everything that your semantics allows you to fix, we are told, and I'll show you that your interpretation still admits of indefinitely many *understandings* (ways for the world to be such that the sentence would be judged, by competent speakers, to be true, or false).<sup>58</sup>

What the semanticist assumed when she formulated the CET was that expressions could somehow be imbued with the magical powers to determine not just their referents but their senses too, thus blocking the slide into indeterminacy.<sup>59</sup>

In short, the expansion process could terminate *only on the assumption that the expressions in the expanded sentence could not be misunderstood*. But that's not what words *are*, the argument goes.<sup>60</sup> Guesswork, that is, is not an incidental feature of NL, but rather it is an essential property of any sign-like (content-bearing) entity.

In contrast to this, Frege's (1906: 315/384) idea was that "the word 'interpretation' is objectionable, for *when properly expressed*, a thought leaves no room for different interpretations" (my emphasis).

The idea embodied in CET, then, was that when we finally get a sentence expressing a complete thought we have reached *the end stage of interpretation*.<sup>61</sup> Thoughts, on this view, cannot be interpreted, be-

<sup>57</sup> As Searle (1980: 231) concluded.

<sup>58</sup> See e.g. Travis (2006b: 114) and my remarks in note 51.

<sup>59</sup> Wheeler (2000: 59-60) has an insightful discussion of this misguided conception of meaning. A rather extreme form of this view can be found in the *Cratylus*. Kevin Van Anglen informed me that the Platonic conception of naming was taken up by the American transcendentalists.

<sup>60</sup> For a conclusion along these lines see Carston (2002: 42).

<sup>61</sup> Rorty (1989b: 65) is a good antidote to this Fregean idea.

cause they are the *outcome* of interpretation.

And yet if semantic underdeterminacy is indeed a constitutive phenomenon of our system of signs, that Fregean idea seems deeply incoherent. In Derridean terminology, signs are essentially *iterable*; that is, they allow for (and indeed *demand*) cross-contextual projection (curiously enough, that was in fact one key component in the Chomskyan view of the CC), they cannot be kept confined to univocal significance *even within a single context*.

With this in mind, the attempted flight from context embodied in the CET can then be seen as a self-defeating effort to neutralise iterability (if you try to do *that*, you destroy the very essence of the sign).<sup>62</sup>

And it's not even clear that at any stage in the expansion process the sentence is any *less* ambiguous than at any previous stage (at best, it might be so only with respect to a specific word, but the form of words used to reduce *that* ambiguity will generate new branches and as the sentence grows in complexity, so does ambiguity).<sup>63</sup>

For these reasons, I will assume that the sketch of the proof against the CET is along the right lines.<sup>64</sup>

In any case, even if the terminating process *could* be completed, there are reasons to think this would provide little comfort to the semanticist. Let's see why.

### 3.4.2. Essential Indexicality

Let's now suppose that the expansion process can terminate. It seems clear from the preceding considerations that *if* it did terminate, it would do so only after a considerable level of complexity had been reached.

Intuitively, if Wittgenstein is wrong and we *can* instead inflate 'this chair is brown' to obtain a sentence where no-one could raise issues of ambiguity, the number of interpretive possibilities that need to be

<sup>62</sup>See e.g. the perceptive remarks in Wheeler (2000: 19), where it is convincingly argued that Davidson, Quine and Derrida are best seen as having attacked the same problem from different directions.

<sup>63</sup>See my discussion on p. 199.

<sup>64</sup>Let me note here that I'm nevertheless impressed by Dummett's (2004: 7) (and Williamson's 1994: 54) idea of a *stiffening of senses* as words are combined together to obtain ever greater precision (or at least: a reduction in the *flexibility* of the atomic senses). A similar line of thought, from a completely different perspective, is to be found in Borer (2005). The idea here is that contrary to the RPA assumption, as we join up words into complex expressions, the additional (grammatical) structure does not increase polysemy, but rather it constrains interpretation. See the text for reasons to question this move.

ruled out for this to occur is very great indeed.<sup>65</sup> It seems clear that we would need to add a further clause *for each open possibility* until they are *all* closed off.<sup>66</sup>

And so it seems as if the outcome of the *successful* process of expansion would be a sentence of such complexity that:

- i) we might well have difficulty in parsing it, let alone interpreting it (remember: the purpose of the CET claim was to show that in uttering an NL sentence we are really *knowingly* expressing the fully telescoped content);<sup>67</sup>
- ii) we might have difficulty in establishing that *it means the same* as the original sentence (after all, wasn't the initial problem an *interpretive* one? how can we know that the expanded sentence means the same as the original one if we were having trouble finding out what *that* meant?);<sup>68</sup>
- iii) we might have difficulty in establishing the needed *cognitive equivalence* of the two sentences if the *content* being elucidated via the expansion process is to play the needed role in our reasoning (recall the Fregean admonition: no piece of content can serve as an item in our thinking unless it is fully determinate).<sup>69</sup>

One particularly acute aspect of the problem is the essential egocentricity of much of our thinking. On the face of it, indexicality seems to be ineliminable. The expanded sentence would have removed not just any trace of contextual dependency but also any trace of recog-

<sup>65</sup>Quine (1960: 227) raises an interesting possibility: that we do not need a *full* expansion for the purposes of logic and reasoning (eternal sentences are only required for science to state its laws). All we require is *enough expansion* to secure validity. Shapiro (1991: 210) takes a similar line on the question of the intended interpretation for first- and second-order logic. Disputes are stopped when we realise there is no point in taking sharpening procedures any further. I discuss a strategy of this kind in chapter 5. One obvious immediate worry concerns the vagueness of the 'enough' qualifier. A second worry is deeper: of course we *routinely* agree that no further disambiguation is needed at a given context. But our difficulty is explaining *why* we think so and whether we are *justified* in so doing.

<sup>66</sup>Think of all the (non-trivial) ways for something to be appropriately referred to by means of a token of 'this', all the ways for a thing to *be* (note: *not* to count as) a chair, for things to be brown chairs and so on. Of course, Chomsky (2000: 20) denies that these matters are "appropriate for naturalistic inquiry", and I agree. I think we should however draw the appropriate conclusions with regard to the nature of content and linguistic structure (I try to do that in chapter 6).

<sup>67</sup>This is so because it is an essential part of the *modus tollens* I mentioned at the start that CET *explain* the phenomenon of shared content. Communication is made possible by our sharing fully disambiguated content.

<sup>68</sup>Note that talk of *intended* interpretation here serves no purpose: the difficulty lies precisely in making clear *which* interpretation we meant.

<sup>69</sup>I'm not entering into the question of whether same-meaning, same-saying and cognitive equivalence are co-extensional notions. Perhaps *ii*) and *iii*) come to the same thing, perhaps not.

nisably *indexical* content.<sup>70</sup>

Familiar considerations from the literature that followed Dummett (1973: 384) and Perry (1977: 9) suggest that removing indexicality (*de-centering* our thought, so to speak) would alter the cognitive content (and the causal properties) of the original sentence to the point that a theory of meaning that relied on the CET would *not* be a theory of *our* meaning at all (once the indexical-laden sentence is de-indexicalised, we may no longer recognise its informational content as relating to *us*), but of a (magical, non-iterable, non-polysemic) language for gods; and what use would *that* be to *us*?

The semanticist will again complain that we are confusing the tool and concepts needed by the *theorist* to model competence with those *actually employed* by the practitioner—the expanded sentence belongs to the theoretical apparatus only and any worries about its cognitive content are misguided.<sup>71</sup>

This reply however won't do. The problem we started from was an *interpretive* one, the problem of how we can secure a determinate content for the sentences *we* use in our everyday exchanges in the face of all-pervasive polysemic phenomena.

CET was meant to address that problem by positing hidden structural complexity that *we* were *somehow* tacitly exploiting to secure an interpretation—and remember: the all-too-common reply to the RPA is that *without an assumption of determinacy* for the pieces of content we *actually* exchange in conversation it would be a miracle that communication *can* occur at all, and only the truth of CET can ensure the semanticist's *modus tollens* move will go through.

The CET-semanticist however is simply replacing one miracle with another (the incredible idea that we *can* implement the expansion process and that we *could* understand the fully telescoped translation

<sup>70</sup>This is a point that Russell (1957: 120) spectacularly missed in his caustic CET-based reply to Strawson (1950). An early version of the CET with respect to indexicality is in Frege (1897a: 135): "A thought can be clothed in a sentence that is *more in keeping* with its being independent of the person thinking it"—my emphasis, for that seems to suggest some hesitation on Frege's part that the job *can* be carried out; indeed, in Frege (1882: 72/51) he speaks of the "unbearable [*unerträglich*en] prolixity" of the expanded version (no sign could *carry* the weight of the fully expanded material).

<sup>71</sup>Something like this reply is at play in Quine's (1960: 227) demarcation between the language of science and that of the vernacular. See also Russell (1957: 125) and Stalnaker (1984: 41, 63). In discussing Stalnaker's diagonal proposition, Perry (1979: 43) draws a distinction between classificatory purposes with respect to propositions and beliefs versus content-fixing notions. One could use that distinction to argue that CET is a claim about classificatory efforts (the language of science) and not about content strictly taken (and as expressed in the vernacular). The point remains: CET makes a *false* claim *even with respect to the descriptive needs of the theorist*, since the theorist too speaks in a language.

expressing the thought encoded by the sentence).<sup>72</sup>

Securing determinacy was supposed to be a *real* fact about our linguistic practices, not something that only belongs in the theoretician's laboratory.

And the fact is: if the expansion process could terminate, the cognitive profile of the resulting content would be such as to make it unfit for purpose. It couldn't serve as the object of the attitudes; it couldn't serve as the stuff we reason with; it couldn't preserve the egocentric informational content that is essential to our thinking.<sup>73</sup>

My conclusion is thus that CET lands the semanticist with yet another dilemma:

SD V: Either the process of semantic expansion to secure content-determinacy terminates or it doesn't. If it does, the content will be cognitively unmanageable; if it doesn't, determinacy is never secured at any of the intermediate stages. Either way, determinate semantic content is not available for meaning-computation.

Despite its initial appeal, then, I think we must conclude that CET is a non-starter. A different conception of semantic content is needed, and I'll return to the issue in chapter 5 and 6. It's now time to get the pragmaticist sweating a bit.

### 3.5. Grice's Circle: A New Dilemma for Pragmaticism

CET was the semanticist's attempt to ensure determinacy of content.

Now, what is the story told by the pragmaticist? Well, there are many such stories, but here's one to get us started: a sentence expresses "a sort of common core of meaning shared by every utterance of it."<sup>74</sup> Linguistic competence allows us to decode that core meaning, but what we get at that stage is merely sub-propositional (i.e. non-truth-evaluable) content.<sup>75</sup>

<sup>72</sup>Those impressed by anti-pragmaticist replies of the sort given in e.g. Bach (2004: 38) will insist that I am confusing semantic and communicated content; indeterminacy, though, begins *already* with semantic content, however minimally conceived. Unless we understand what one is saying semantically, we cannot understand what one is communicating pragmatically (however broadly we take this latter notion).

<sup>73</sup>Could the semanticist retort that *what* we understand at a context of utterance is the character-level material? Obviously not. If you tell me you're cold, *that* is what I understand, not that the speaker is cold at the time of utterance and so on. It is essential to semanticism that it claim that NL sentences *go proxy* for fully expanded content.

<sup>74</sup>Sperber and Wilson (1986/1995: 9).

<sup>75</sup>Note the difference to Cappelen and Lepore's (2005a) fully propositional conception of mini-

To get to a fully truth-evaluable interpretation of the utterance, we need to mobilise *pragmatic* competence, i.e. the ability to perform inferences, on the basis of semantic content and contextual factors, to the speaker's intentions. Semantic content is (nothing but) *evidence* for those intentions.<sup>76</sup>

Language mastery, on this picture, is a *hybrid skill* whose dominant feature is the ability to infer *to* determinate content from an indeterminate basis. The (partial) basis for such inference is the (subpropositional) semantic content expressed by the tokening of the sentence (so, decoding linguistic material is, generally, subservient to inferentially-driven interpretation).

This is, very roughly, the (naturalistic) picture given by Relevance Theorists (and, with substantial differences, by other pragmaticists too—the representational scope of semantics is severely thwarted and substantial pragmatic intrusion into semantic content is also argued for).<sup>77</sup>

I hope the sketch above suffices to give a sense of the contrast. On the semanticist picture, language mastery is essentially a question of internalisation of rules that determine a semantic representation which is either fully propositional or sufficiently precise to *determine* propositional content.

The semanticist is happy to concede that there *are* inferential processes involved in the interpretation of utterances, but these typically occur *after* we're done with the processing of *semantic* content—they do not *threaten* the autonomy of syntactico-semantic devices in the representation of content (to think otherwise is to blur an important boundary between semantics and pragmatics, they add).<sup>78</sup>

Let's note a few more points of contrast: the pragmaticist rejects LF-indexicalism and CET more generally (*semantic* representations do *not* correspond “very closely” to thoughts)—*contra* Kaplan (2005:

mal semantic content here (but see the remarks in Cappelen 2008: 269-70). Note also that according to the pragmaticist, the Carnapian starting point for evaluation has not yet been reached when linguistic content is decoded: semantics is a prelude to pragmatics in a very strong sense on this view (Turner 1999b: 15).

<sup>76</sup>Sperber and Wilson (1986/1995: 23, 189).

<sup>77</sup>For all the frequent talk of psychological rooting of the communicative process in Sperber and Wilson (1986/1995), it seems to me that their picture is not (directly) vulnerable to Salmon's (1991) pragmatic fallacy accusation. For they make a very clear separation between semantic content and communicated content. Where the semanticist and the relevant pragmaticist part company is with regard to what counts as *semantic* content: the latter will treat cases of content expansion (such as the transition from 'I've eaten' to 'I've eaten today') as inferences, whereas the former will insist it is still a case of *syntactically* constrained *decoding* (to use Sperber and Wilson's favourite terminology).

<sup>78</sup>See for instance Bach (2004).



939), the semantic content of a sentence(-type) does not suffice to determine *which* thought its tokenings express;<sup>79</sup> for the semanticist (and the Chomskyan), there are instead intrinsic properties of expressions (and of linguistic structures) that *guide* our apprehension of how to use language to *represent* the world and these properties are *fully formal*.<sup>80</sup>

And *these* are the crucial features of our connection with the world as we represent it in language (language is not a mere stepping stone towards a cognitive commerce with the world that is dominated by inference).

Not so for the pragmaticist: what is truly peculiar to *human* language (and communication) is not the unbounded generation of (sub-propositional) strings but rather *the inferentially guided recognition of intentions*.<sup>81</sup> And *that* can happen even in the absence of an underlying code.<sup>82</sup> It is pragmatic competence, not syntactico-semantic competence, that defines us *qua* masters of a language, then.

In short, on the pragmaticist view, what cries out for explanation is not the CC as viewed through compositional glasses (it's not grasp of structure that *explains* our grasp of novel *thoughts*) but rather the fact that, to put it in Churchillian terms, *so many thoughts owe their communicability to so few sentences*.<sup>83</sup>

On this picture, it is indeed the *independence of thought from its expressibility in language* that is striking (compare that to Chomsky's fascination with the independence of *language* from context!).

What should give pause to the theorist, the pragmaticist seems to tell us, is the hiatus between the unboundedness of thought and the paucity of available sentences. And *how* we succeed in communica-

<sup>79</sup>Sperber and Wilson (1986/1995: 9-10). Note that their claim is not (exclusively) about communicated content: on their view, semantic content is *never* thought-like. The same claim is made in Carston (2002: 20, 83).

<sup>80</sup>See e.g. Lasnik and Uriagereka (2005: §1.4). Indeed, for the Chomskyan minimalist, the linkages between expressions and their meanings are "perfect" (language is optimised towards that purpose).

<sup>81</sup>Sperber and Wilson (1986/1995: 25). Pragmaticists are *deeply* divided on the issue of the role of inference. On the Relevance Theory view, all meaning is inferentially derived. For Recanati, by contrast, there is non-inferential pragmatic encroachment on semantic content.

<sup>82</sup>Sperber and Wilson's highly questionable term for language. Terminology aside, note the points of contact with Davidson (1986) here.

<sup>83</sup>For Carston (2002: 76), "mentalese must have a large stock of concepts that are not encoded by any element of natural-language form". The discussion in Sperber and Wilson (1986/1995: 192-3) makes it clear that on the RT account we do not think using sentential semantic representations. We think using fully-fledged (ego-centric) thoughts, automatically enriching the assumption schemas encoded in sentences. We are not even *aware* of the linguistic material employed in the exteriorisation of thought—note how CET is at work even in the (non-radical) pragmaticist camp.

tion *despite* that (radical) independence is the fact that we ought to find puzzling.

The pragmaticist answer is that we do so by inferring *to* intentions via the broad maxim of *relevance* (we home in on intentions through considerations of optimal relevance).<sup>84</sup>

This is the claim I want to test in the remainder of this chapter.

### 3.5.1. Narrowing Down Interpretation

My thumbnail sketch hardly does justice to the many flavours of the pragmaticist position, but it should do as the groundwork for my dilemma.

Whatever the specific implementation, the basic idea behind the non-semanticist approach to interpretation is that the process of determining *which thought* a sentence expresses is *generally* unconstrained by semantic content.

As is clear from the recent Wilson and Carston (2007: §2), on this view lexical pragmatics *always* involves inference (no matter how familiar the semantic input), and the overall interpretive process is one whereby we are *progressively narrowing down denotation*.

Inference, however, treats semantic content as one more piece of *evidence*, not as a premise in a deductively disciplined meaning derivation; not, that is, as *the* determinant of truth-evaluable content.

The basic conception of semantics, then, is at bottom the same as the semanticist one—the aim is to nail down denotation. But for the pragmaticist the job cannot be carried out *within* semantics (or the language faculty): and, crucially, as we move beyond the resources of that faculty we lose MDP.<sup>85</sup>

Here's the question that worries me: is this view of content individuation *internally* coherent?

And I now want to put pressure on the pragmaticist conception of content individuation from, I think, a different perspective than is usual in the literature.

Roughly speaking, the charges against pragmaticism centre on three broad lines: that the account *overgenerates* (it predicts a range of completely unrestricted interpretations that are *in fact* unavailable);

<sup>84</sup>The details are in e.g. Sperber and Wilson (1986/1995: ch. 3) and Wilson and Sperber (1993). I focus on Relevance Theory, but my argument, if it is any good, will apply to pragmaticist positions more generally. Truth-conditional pragmaticism is in any case affected by my anti-semanticism argument as well.

<sup>85</sup>There is also a more radical version of pragmaticism, namely Travis', where semantic content is again very minimal and what settles questions of truth or falsity for sentences is the *understanding* that competent speakers consider reasonable to attach to the expressions therein.

that it is hopelessly circular, and that it is, as we may put it, *externally* incoherent (pragmaticists could not even state their own thesis).<sup>86</sup>

My worry is different. It concerns the *architecture* of the pragmatic account of interpretation. We are told that semantic content does not determine a fully truth-evaluable proposition. It just instigates a *search* for the most relevant (or most reasonable) interpretation among a range of candidates that are ranked according to their relevance (or reasonableness).

And that's precisely where my worry originates: the pragmaticist tells us that we *need* inference to make content precise (before pragmatics steps in, there is no determinacy; semantics all on its own cannot fix content uniquely).

We are also told that speakers assess and, crucially, *identify* assumptions, conversational interests and any other contextual factor that may help *rank* interpretations according to their relevance.<sup>87</sup>

Presumably, the *content* of these assumptions, interests and so forth is itself determinate—or else: how could we *identify and rank* them?

If the pragmaticist is right, however, it is obscure how identification (and consequent ranking) of assumptions and interpretive hypotheses can so much as take place. There are, *ex hypothesi*, no candidate pieces of content to be individuated prior to *extra*-semantic reasoning being carried out.

But *inference* is an eminently *rational* activity. Inferential moves, whether deductive or inductive, are regulated by standards of rationality (given certain grounds, certain steps are licensed by those grounds under the rules currently in force). Moreover, good inference is truth-preserving. Without determinacy of content, though,

<sup>86</sup> A claim made in e.g. Cappelen and Lepore (2005a: ch. 9). I am going against their terminology: for them, pragmaticists are internally incoherent in that they can't state their own thesis. For me that's a meta-theoretic, and thus external, problem. Incidentally, I do find their critique of pragmatism slightly off-target. For one thing, they appear to misread Travis. On Travis' view there aren't many different properties picked out in context. Rather, for every predicate-expression there is a *single*, minimal property denoted by it. What varies with context is what it takes to satisfy *that single property* (compare truth relativism v. truth pluralism, or versions of pluralism according to which there is a single property of truth whose satisfaction criteria are domain-specific: Travis-properties are non-domain relative with domain-relative satisfaction conditions). Secondly, pragmaticists can respond that the Background against which their theories are examined will make their claims expressible (and determinate). I see no incoherence in *that* claim. For criticism of the RPA along the first two lines, see e.g. Dummett (1989), Kamp and Reyle (1993: 9-11), Bach (1999b: 80), Levinson (2000: 257-58) and Salmon (2004b: 346).

<sup>87</sup> See Sperber and Wilson (1986/1995: 118, 165-7, 183, 189, 257) and Carston (1988: 169). Levinson's (2000) account gives great prominence to the notion of *default rankings* among candidate interpretations.

there is no assessing for truth-value.<sup>88</sup>

It is therefore completely obscure, and indeed I maintain: it is deeply incoherent to describe the interpretive process as a *rationally constrained choice* among previously ranked possible interpretations where, by the pragmaticist's own lights, *we cannot individuate content until pragmatics kicks in*.

At best, what we have here is a *cognitive leap of faith* from indeterminate (and indeed unindividuated!) premises to a (mysteriously) determinate conclusion (an abrupt transition from reasoning about variables to reasoning about a constant without *any* justification for the instantiating move).<sup>89</sup>

For Levinson (and Dummett), the circle facing the Gricean was to do with the fact that the *mechanisms* that individuate propositional content were substantially the same as those required to fix *post-semantic* content—and this threatened the tenability of Grice's distinction between semantically and pragmatically determined content.

My worry is however not about *procedural* circularity, but rather about the possibility of securing *determinacy* of content by pragmatic means—the worry kicks in because of the (no doubt plausible) claim that (*contra* Grice's own view of these matters) pragmatic input is *not* limited to the *calculation* of post-semantic implicatures but it is rather essentially involved in the *generation* of content (and for my purposes it doesn't much matter whether this is claimed to take place in parallel with or prior to the semantic module doing its job).<sup>90</sup>

As I see matters, the trouble for the (neo-)Gricean is that she lacks the very materials needed to carry out the interpretive tasks: if her story is right, she has no material for even stating her rules of inference, individuate interpretive candidates *as given in thought*, and carry out the inferential process as a whole.

<sup>88</sup>Note that the familiar technical amendments (e.g. supervaluationism) to classical semantics are of no use here: we are dealing with indeterminacy of *sense* not of reference here.

<sup>89</sup>See Evans (2009: 106-07) for a move of this kind. Note that for the pragmaticist, we move from non-truth-evaluable content (the linguistic representation) to truth-evaluable content (to interpretations, that is, to "conceptual representations of thoughts"—Blakemore 1992: 30) *without* sufficiently precise linguistic constraints. The only thing that disciplines the transition is *further* representations in thoughts, co-operative principles and laws of inference. But can *any* of these notions be made stable *purely by communicative means*?

<sup>90</sup>The truly groundbreaking pragmaticist turn took place in Sperber and Wilson (1986/1995: ch. 4, §2), where the notion of *explicature* is first introduced. Care is required here: Sperber and Wilson (1986/1995: 257-8) make clear that for them linguistic semantics operates on sub-propositional content and so they can legitimately view their conception of pragmatic input as applying to *post-semantic* content. The crucial point for my purposes is that on their story there is no truth-evaluable content until pragmatics enters the scene.

Just reflect on the crucial notion of inconsistency that she requires: to rank candidate interpretations we have to test for inconsistency (so as to screen off interpretive choices incompatible with established facts about the context), and we cannot do that if we haven't got fully determinate propositional content *given that the process of content completion is essential to the consistency test*.<sup>91</sup>

Note that while the problem for the CET view was that the interpretive procedure *could never terminate* (for familiar Wittgensteinian considerations), the difficulty confronting the pragmaticist is that the procedure *could never get started*.

And if the pragmaticist replies that we *can* secure *ad interim* determinacy of content to get the game started (with the procedure itself narrowing down content as it computes the pragmatically derived values), the semanticist can retort that if *that* is at all possible then it's unclear why *she* was supposed to be any worse off than the pragmaticist—if we can take for granted that there *is* a level of content which *is* determinate enough to individuate candidate interpretations, why should we despair that *linguistic* content could not be made at least as determinate?

The pragmaticist often appeals to mental content (and some form of the language of thought hypothesis) as what guides our inferences, but surely we have no more secure way to nail down *mental* content (of any non-basic kind) than by giving a linguistic description of it.

The thing is, while the pragmaticist framework needs *determinate* pieces of content waiting in the wings as we move from linguistic sub-propositional content to fully truth-evaluable content, the range of candidate interpretations available in thought could be *no more determinate than the sentences used to describe them*.<sup>92</sup>

<sup>91</sup>There are multiple ranking processes in the RT framework: sentences are “sets of semantic representations” and there are as many such representations as there are ways for the sentence to be ambiguous (Sperber and Wilson 1986/1995: 193). Note already one first tension here: ambiguity is ambiguity *in the fully expanded content*, yet it is *identifiable as the ambiguity it is* already at the level of assumption schemata; the representations themselves are *at best* “fragmentary” and “incomplete” representations of thoughts. So in turn each of the (first-level) disambiguated representations will then be further enriched (expanded, narrowed) according to the communicative intentions in a particular context. We thus have at least three layers of listing and ranking according to the principle of relevance and the various maxims inherited from the Gricean framework. One may reply that all along we merely identify incomplete logical forms (the notion seems perfectly coherent): but in the very same page we are told that these forms are *never* present to consciousness. The rankings are *always* done on the fully explicated content. I find this picture deeply flawed for the reasons discussed in the text.

<sup>92</sup>How else but by means of words could we represent and *rationaly* discipline the inferential relations holding between thoughts? I am aware externalists about content will complain that I am over-intellectualising the conditions for grasp of content. Maybe. But I am holding my ground

And if so, we might just as well concede that language *is* able to express definite content *even before pragmatic inference steps in*.

The dilemma for the pragmaticist, then, is that when donning her negative, anti-semanticist hat she has to argue for a notion of (terminally underdetermined) linguistic content that cannot serve the purpose she requires of it when it comes to elaborating her alternative conception of interpretation.<sup>93</sup>

Ultimately, the problem for the pragmaticist originates from her claim that semantic content is underdeterminate *and* ambiguous.

The first claim asserts the essential schematicity of semantic content. But the second claim, in the reading usually given by the pragmaticist, is that the sentence *as it is* could express indefinitely many thoughts which are *themselves* determinate.

One example may help: for the pragmaticist, a sentence like ‘Rupert bought the *Times*’ is not yet truth-evaluable, it expresses no determinate content.<sup>94</sup> We are then told *which* meanings it could carry (which thoughts it could express), and this is done by adding *further expressions* that (supposedly) settle the issue. ‘Rupert bought a copy of the *Times*’ and ‘Rupert bought the press enterprise that publishes the *Times*’ are thus given as two candidate *interpretations* of the original sentence (presumably entertained in thought).

The conclusion, we are told, is that by using Gricean maxims and inferences, it is possible “to convey an unambiguous thought by uttering an ambiguous sentence.”

What I find profoundly baffling in this account is that the *sentences* used to *distinguish* available interpretations are supposed to be *unambiguous* (as if, for instance, in the example above we could entertain no doubt as to the meaning of ‘a copy of’!).

But, as I’ve said already, if there was a *genuine* problem of *semantic* ambiguity at the first-order level, it is astonishing that the pragmaticist should think that by using *semantic* means (by recourse to *more sentences*) we could succeed in fixing content.

In effect, the relevance theorist’s account of interpretation is but a version of CET where it is pragmatic inference (and not semantic rules) that ensures termination.<sup>95</sup>

anyway.

<sup>93</sup>Here I make contact with a strand in Cappelen and Lepore’s (2005a: 132) critique of radical contextualism.

<sup>94</sup>Adapted from the example discussed in Sperber and Wilson (1986/1995: 34).

<sup>95</sup>A rather clear confirmation that I’m right in this diagnosis comes from Carston (2002: 76) where we are told that thoughts are “the *result* of utterance comprehension processes”. Exactly, it will be remembered, Frege’s own view of thoughts as the outcome of interpretation. The disagreement is purely instrumental here: the Fregean (but possibly not Frege himself!) will insist that the

So, whereas on the semanticist front semantic underspecification is a *formal* problem, increasingly dealt with by mobilising additional *semantic* resources,<sup>96</sup> on the pragmaticist side the theoretical burden is transferred to the level of thought: “linguistically encoded semantic representations”, we are told, are “abstract mental structures which must be inferentially enriched *before* they can be taken to represent anything of interest.”<sup>97</sup>

Now, that seems to be a claim about *all* linguistically encoded content—*anything* that is *linguistically* encoded is *representationally deficient*, that is, no matter whether it is encoded in a sentence being interpreted or in a range of sentences in the *language* of thought being entertained as its possible interpretation.<sup>98</sup>

Hence what I consider to be the deep incoherence of the position: it is a sheer fantasy to think that a conception of irreducible *semantic* underdetermination could be compatible with the claim that there is a range of candidate interpretations endowed with determinate content which are available for (rational) ranking and selection (and which the original sentence ambiguously denotes and its expanded counterparts make precise) *prior* to (by the pragmaticist’s own arguments) *any* possibility of content determination.<sup>99</sup>

In a nutshell, the incoherence is in the idea that by moving from linguistic meaning to communicated meaning, the meaning (and the metaphysics) of ‘meaning’ somehow changes (it *magically* moves from incompleteness to completeness).

interpretive process is wholly regulated by syntax, while for the pragmaticist what secures determinacy of content are “powerful pragmatic inferential mechanism[s]”.

<sup>96</sup>See e.g. Reyle (1993), van Deemter and Peters (1996), van Eijck and Jaspars (1996), Crouch and van Genabith (1999), Jayez (2001), Blackburn and Bos (2005)

<sup>97</sup>Sperber and Wilson (1986/1995: 174). My emphasis.

<sup>98</sup>Note a difference between Sperber and Wilson (1986/1995) and Carston (2002: ch. 5): for the latter, but not for the former (although see Wilson and Carston 2007), linguistic forms point to items in mentalese that are *themselves*, in the first instance, schematic (although less so than the linguistic representations that triggered the interpretive process). Pragmatic inference then narrows down the profile of the mentalese sentence until the appropriate representational content is fully *constructed* as the interpretation process reaches its terminus. For the purposes of my argument, this only adds one intermediate step to a picture that remains incoherent for the reasons rehearsed in the text.

<sup>99</sup>Carston (2002: 75) speaks of the polysemy of NL expressions as their having “missing bits” which a translation into mentalese fills in appropriately. Note that for her (p. 98) it is important that we preserve the autonomy of (sub-propositional) semantics. And so she insists there is no circle, contra Levinson, because the division presupposed by Levinson is wrongly drawn: semantics does not reach as far out as Levinson claims. Once we draw the boundary properly, the circle disappears. My claim is that even when the boundary is drawn where the pragmaticist wants it, my argument will still go through. For anything but the most rudimentary of belief-states, we can make *no* sense of *precisely drawn* content other than in linguistic terms, hence the dilemma.

Where the (non-Travisian) pragmaticist goes astray, then, is in the idea that defending the determinacy of thought is compatible with denying the determinacy of semantic content.

The semanticist relied on a magical conception of language to implement CET by openly semantic means; the pragmaticist, however, relies on no less magical a conception in her view of the language of thought as something other than what it is, namely, and precisely, a *language*.

### 3.6. Conclusion

We have examined two opposing views of content: on the semanticist view, we seem forced to espouse some form of CET, which in turn seems to force on us the impossibility of ever expressing content in a fully determinate manner because to defeat polysemy and underdetermination the meaning-specifications would need to be infinitely long (or, if finitely specifiable, computationally unmanageable); the pragmaticist side, on the other hand, seems to be committed to a variant of the CET which discloses an internal incoherence in its conception of meaning individuation.

As we saw, the instability of the pragmaticist view derives from the odd coupling of Wittgenstenian distrust of first-order (as it were) linguistic meaning with anti-Wittgenstenian blind faith in *i*) the powers of sentences in the language of thought to *mean* determinately (Carston 2002: 74) and in *ii*) the idea that we *can* infer to the mental states of others with *that* very same degree of determination (Carston 2002: 83).<sup>100</sup>

That coupling is incoherent: if we buy the Wittgenstein/Derrida line on polysemy, its force will carry over to *any* linguistically structured network of content-bearers.

Both semanticism and pragmaticism make the remarkable move of saying that while the surface structure of NL lacks the ability to confer truth-conditions on its expressions, there is a hidden layer of content-bearing entities (logical forms in one case, Mentalese sentences in the other) endowed with all the neat, tidy properties of compositional semantics that are denied to, *of all things*, our *natural* language, our

<sup>100</sup>The incoherence in Carston's (2002: 74) position is brought out by her claim that mentalese sentences have truth conditions *because* there is a *specifiable* state of affairs that (can) make them true. Carston (2002: 257) clearly thinks that a truth calculus can only be devised for the language of thought. The point is: specifiable *in what language*? I'm equally unconvinced by the conception defended in Fodor and Lepore (1999), which Carston cites approvingly.



trusted and friendly vernacular.<sup>101</sup>

I think that both views, to the extent that they are committed to a version of CET, end up making a mystery of language mastery. They both start from the (presumably disappointing) realisation that ordinary language is not *magical* (in the sense of: able to secure—and signal—reference unerringly),<sup>102</sup> and then move on to posit a *different*, presumably *extra*-ordinary language that enjoys the magic powers that elude NL (a ‘formal’ one for the semanticist, or mentalese for the pragmaticist).

My own view is that a proper understanding of the routes that connect language and world (the routes *to* and *from* linguistic sense) requires the abandonment of the idea that there is *any* language that is suitably magical. As should be clear by now, I think proper reflection on these issues forces on us a radically minimal conception of content (both linguistic *and* mental).<sup>103</sup>

I will take up this question again in chapters 5 and 6. After my examination of compositionally-determined meanings and the meaning of atomic sentences, I now need to turn my attention to the meaning of the logical constants themselves.

<sup>101</sup>Indeed, that (relevance) pragmaticism is but another implementation of CET is shown by Carston’s (2002: 75, 82, 93 fn. 39) claim that (non-indexical) mentalese sentences are eternal (i.e. maximally non-ambiguous). Carston (2002: 83) also claims that even where mentalese sentences display context-sensitivity *and* underdeterminacy, that is, even when thoughts are *incomplete*, they are not so to the same extent as NL sentences. Why?

<sup>102</sup>‘Magic language’ is Wheeler’s (2000: 3) apt term.

<sup>103</sup>One might object to the main line of this chapter, and indeed of this dissertation, that it relies essentially on some notion of sense given in terms of *possession conditions* as opposed to *attribution conditions*, and being operative in both the semantics and in our account of competence—see e.g. Peacocke’s (1992: 29-1) complaint against Schiffer (1987). Jettison that notion (or distinguish sharply between possession and attribution conditions) and all will be well (we can give an account of basic meaning-practices in e.g. purely causal-deferential terms). I grant the distinction is a good one, but I think the objection would be misguided. First, we need a notion of sense-as-possession-conditions to account for competence as a *rational* activity; secondly, causal-deferential accounts merely shift the problem: *what* explains competence in the case of the experts (or of the community as a whole)?—I assume here that deference is a well-founded relation.

## Chapter 4

### No *Buts* and *Ifs* Pragmatics and the Logical Constants

#### 4.1. Introduction

In the previous two chapters I have argued that complex meanings are only provisionally projectable from the lexical base and that the meaning of atomic sentences is ineradicably indeterminate.<sup>1</sup>

In this chapter I shift my attention to the meaning of the compositional operators, the (non-intensional) connectives.

I pose two main questions: whether the NL connectives match their logical counterparts and whether they are immune to indeterminacy worries. My conclusion is broadly negative on both counts. For convenience's sake, and without loss of generality, I choose conjunction as a case study.

##### 4.1.1. UaGS, the Connectives and Representationality

It was part of the Tractarian framework that logical words were not representational at all, and the Tarskian clauses standardly employed in truth-conditional semantics do somehow inherit that *emptiness*. Uniquely among expressions, sentential connectives refer to truth-functions (both their arguments and their values are truth-values) and not to worldly extensions (or intensions that contribute to the

<sup>1</sup>And to repeat: I take both conclusions to be mandated by standard semanticist assumptions. Incidentally, I have to confess to a lot of sloppiness regarding the use of 'meaning' in this dissertation. More often than not, I'm using it *under semanticist assumptions*. My own conception is much thinner, of course, because I deny MDP. I hope, ahem, context makes clear which sense is being used on a particular occasion.

determination of such extensions).<sup>2</sup>

Indeed, the mark of their logicity is that, as e.g. Bell and Machover (1977: 6) point out, they become “part of the [logical] form” of expressions. Logical constants are thus ‘taken out’ of language, as it were, and absorbed right into the form of compound expressions (and the key idea behind UaGS was precisely that our understanding of complex expressions was *mediated* by grasp of the logical components embedded into their form).

Apprehension of the meaning of the connectives is thus not separable from apprehension of the form of the complex expressions whose main connective they are.

Clearly, it is the representational inertness of the logical constants that engenders their content- and context-insensitivity, and it is this feature that makes logic a topic-neutral discipline (the inferences sanctioned by the rules for the connectives obtain no matter what we substitute for the non-logical vocabulary involved).

To those trained in the Frege-Tarski-Montague tradition, it has seemed natural to assume that the Boolean connectives have fairly close (albeit messier) counterparts in a pretty obvious class of NL sentential connectives that also inherit their context-inertness.

In that tradition, any divergence between the behaviour of the two classes is accounted for in pragmatic terms. In particular, any inference that we might make beyond those licensed by the elimination and introduction rules for the Boolean connectives (say, to the conclusion that a certain *worldly* relation holds between the conjuncts) is seen as justified, if soundly drawn, solely on the basis of pragmatic factors. It is pragmatic noise, then, that makes it look as if NL connectives display, on occasion, non-logical features.

Appeal to pragmatics, however, creates another dilemma for the semanticist. If we explain departures from the Boolean meaning by blaming them on the pragmatic content of the expressions involved, we face the *Scope Problem*, the apparent phenomenon whereby pragmatically-determined content gets captured under the scope of *logical* operators.

Those operators, however, were supposed to operate on semantic content alone. Defending the thesis that NL connectives are Boolean by appeal to pragmatics will therefore bring pragmatic content under the scope of the connectives. Either way, then, pragmatics intrudes on semantics, *contra* one crucial thesis of semanticism.

<sup>2</sup>This may need refinement depending on the details of the specific semantics. Broadly speaking, it holds for all accounts, however (it is this feature that gives them invariance under permutation).

That is the main problem for this chapter, then: how to keep NL connectives Boolean without weakening the distinctive semanticist claim that semantic content cannot be infiltrated by primary pragmatic processes (processes that penetrate deep into the workings of the compositional machinery).<sup>3</sup>

So, here's the plan for the chapter.

In section 1 I discuss two theses we might hold about the relation between NL and the logical connectives. In section 2 I consider one response to the suggestion that the meaning of 'and' diverges from that of ' $\wedge$ ', one that multiplies the senses for the NL conjunction operator.

Section 3 moves on to examining the opposing strategy. We keep the sense of 'and' univocal and we use pragmatics to explain its non-Boolean behaviour. This raises the already announced Scope Problem. I suggest the dilemma posed by the problem may well force us to concede that 'and' is not immune to indeterminacy arguments either.

In section 4 I discuss ways in which we might blame the truth-conditional variations between NL and logical connectives not on the conjunction operator but on the conjuncts themselves.

Despite my sympathies for an indeterminacy analysis, I nonetheless propose a friendly amendment to a semanticist proposal to keep 'and' Boolean mooted in King and Stanley (2005) by suggesting we view NL conjunction not as a *conjunct-enriching* operator (as on their sketchy proposal) but rather as a *deletion* operator, one that may license pragmatic deletion of informationally-redundant semantic material.

#### 4.1.2. One Problem, Two Theses

So, let's dive straight into the problem. Here's a pair of troublesome cases for conjunction, first mooted by Strawson (1952: 80) and Grice (1981: 186):

- 1) He set to work and found a job
- 2) He took off his boots and he got into bed

The (putative) problem these cases pose for the thesis that NL connectives behave like their formal cousins is that they seem to show that 'and' does not in general respect commutativity (switch the order of the conjuncts in (1) and (2), and the truth conditions of the compound change).<sup>4</sup> If this is a robust enough phenomenon, it seems to

<sup>3</sup>As opposed to the familiar post-semantic inferences posited in the Gricean framework.

<sup>4</sup>I'm ignoring other reasons to think that 'and' is not captured by ' $\wedge$ ': consider the very sentence in brackets in the text; the second occurrence of 'and' there does duty as a replacement for

threaten the postulated inertness of NL connectives.<sup>5</sup>

Now, there are two kinds of questions that we might ask when confronted with these cases.

First, we can ask whether (or to what extent) the semantic clauses given for ‘and’ in terms of classical Boolean conjunction can do justice to all uses of ‘and’ (whether, that is, the truth conditions for NL sentences dominated by ‘and’ are accurately given by the classical Boolean clauses).

This amounts to asking whether NL conjunction is best modelled by classical Boolean conjunction.<sup>6</sup>

The question is thus one of empirical adequacy for our meaning-theory: Is classical logic (CL) the logic *true* of NL (the one properly regimenting its workings)?<sup>7</sup>

Call this the

**NLB Thesis:** the NL connectives are (classically) Boolean.

Secondly, we could ask whether the Boolean connectives can adequately account for all forms of valid reasoning—are they genuinely *complete* or are they leaving out some patterns of inference of high enough generality to deserve to be called logical?

In this case we are asking whether CL is missing out on *crucial* inferential processes carried out in NL that ought to be considered genuinely logical.

Here it’s no longer a case of whether CL accurately models NL, that is, but rather whether the CL connectives are *true* representors of *all*

‘if ..., then...’. Similarly for cases of co-ordinative ‘and’ (where it connects not sentences but NPs or adjectives). See Strawson (1952), Quine (1950), Partee and Rooth (1983), Gazdar *et al.* (1985: ch. 8) and Krifka (1990) for discussion.

<sup>5</sup>It is because of that posited inertness that logicians like to think of connectives as “insulating each component from whatever influences its neighbors might have upon its meaning” (Quine 1950: 57). This, as we shall see, will play a big part in the sequel.

<sup>6</sup>Humberstone (forthcoming: ch. 5) helpfully isolates two ways of looking at conjunction, namely,  $\wedge$ -classicality (the property of a conjunction operator that determines its set of consequences:  $Cn(\{A \wedge B\}) = Cn(\{A, B\})$ ) and  $\wedge$ -booleanness, a property regulating valuations ( $\mathcal{H}_V(A \wedge B) = \mathcal{H}_V(A) \cap \mathcal{H}_V(B)$ )—where ‘ $\mathcal{H}_V$ ’ is the truth-set of a formula). By my use of ‘Boolean’ I intend to refer to both properties.

<sup>7</sup>One question in this connection is: are all *meaning*-inferences (i.e. meaning-analytic entailments holding between expressions) licensed by ‘and’ covered by the Boolean clauses? Specifically, is the inference that the referent of ‘he’ in (1) found a job *after* setting to work warranted by the meaning of ‘and’ or by a pragmatically communicated temporal-causal presumption? See Chomsky (2000: 63) and Beaver (2002: 192) for a defence of meaning-inferences other than those licensed by the Boolean clauses. Note how this question is different to the one standardly posed in the literature regarding the Strawson/Grice cases, for it concerns the set of  $\wedge$ -consequences, not just variations in truth-value.

genuinely logical moves in our thinking.<sup>8</sup>

Call this the

LCB Thesis: the logical connectives are (classically) Boolean.

The question this time is: Is CL the true logic *simpliciter*? Does it cover all cases of logical inference? Does it license pieces of seriously bad reasoning?

We might think that the answers we give to both questions are closely connected (NL is what we use to carry out our reasoning, so one might argue that the two theses above amount to making the same claim—CL *is* the logic true of our reasoning).<sup>9</sup>

Or we might think that NL could be governed by a logic that doesn't have to be CL, without this having any bearing on our view of which logic *is* the appropriate one for *logical* forms of inference—NL is about sublunary reasoning, about the gathering of (possibly partial) *information*; logic is about science as the instrument to limn the, ahem, ultimate structure of reality and it deals with (eternal) *truths* (that's CET at work again).

We may, that is, have reasons to revise the logic *of* NL without having to revise (the one true) *logic*.<sup>10</sup>

For ease of exposition I will split my discussion across two chapters. I'll deal with the LCB thesis in the next chapter (given my doubts about CET I think it's clear where I'll be heading), while limiting myself for the most part to a discussion of the NLB in this chapter.

So, here's our (relatively restricted) question for the rest of the chapter: is classical Boolean conjunction an adequate model of NL conjunction?

<sup>8</sup>Consider e.g. the relevant logician's insistence that in order properly to capture the concept of logical consequence, we require the addition of an intensional conjunction operator (as well as a revision of the conditional)—see e.g. Read (1988).

<sup>9</sup>By 'true of' I mean: 'that appropriately models'.

<sup>10</sup>Another option is to go the Quine (1953b: 150) way and dismiss any departures from the Boolean paradigm as "unwanted vagaries", mere "rhetorical distinctions" that do not really affect truth conditions (1950: 53). The job of the logician-translator is to "distill" the appropriate truth function from the dust raised by rhetorical use. Out of all idiosyncratic uses of 'and' we should therefore extract *the* "minimal linguistic operation" which might be called "simple conjunction" (here I am jumping to a suggestion in Strawson 1950: 81). A properly regimented language will deliver us science, no less, and by means of the translation into CL we have in effect *solved* a problem (Quine 1953a: 154)—that of what to do with an unruly, pragmatics-infected connective. As Quine notes (p.150), the logician never assumes that the NL & formal connectives are synonymous: it is simply that *there is no loss* for the purposes of the logician in leaving behind the NL vagaries (they are, precisely, unwanted).

(1) and (2) seem to suggest it isn't: there are truth-conditional effects on content (a simple switch of the conjuncts) that are not picked up by the Boolean connective (since that connective obeys commutativity and hence is insensitive to the ordering of the conjuncts).

Considerable effort has gone into keeping 'and' Boolean. Broadly speaking, we can partition the proposals into two categories: those that have given a semantic account of the departures from the Boolean paradigm (the proposal here is that 'and' is polysemic—there are many related senses of 'and') and those that have given a pragmatic explanation for the non-Boolean behaviour of 'and' (on this proposal, 'and' remains univocally Boolean in character).

Now, the question arises: which of these options is friendlier to the semanticist project? The connectives, it hardly needs saying, are at the very heart of the compositional machinery. So, a choice *has to* be made here—inaction is not an option. The choice will also entail a commitment to a specific view of the connectives, most notably, a view about *what they can operate upon*.

Well, as I have already announced, yet another dilemma is in the offing for the semanticist, and I'll be arguing that whichever strategy she chooses will bring further grief to her position.<sup>11</sup>

Here's the broad shape of the dilemma. If the semanticist opts for an ambiguity account, she then faces the same problems we raised in connection with CET.

If, on the other hand, the semanticist accepts Grice's (alleged) gift and goes for a pragmatic account that keeps sense univocal, she then faces the *Scope Problem*, which engenders a further dilemma, for as we shall see supposedly *post-semantic pragmatic* content seems to get caught in the scope of the *truth*-functional operators. Under this new dilemma, the problem for the semanticist is that in an effort to keep indeterminacy (i.e. first-level pragmatic infiltration) at bay, she makes her connectives *sensitive* to second-level pragmatic infiltration, thus admitting that there *is* such a thing as pragmatically determined *semantic* content that the connectives can operate upon.

To recap:

**SD VI:** To account for the non-Boolean behaviour of 'and' the semanticist has two options: either she posits multiple senses for 'and', thus facing the same problems raised in connection with CET, or she keeps a single, Boolean sense for 'and' fixed, explaining all departures from the Boolean clauses in pragmatic terms. On the second horn, she then faces the Scope Problem,

<sup>11</sup>Curiously enough, much of the recent debate about 'and' has been a family dispute among pragmaticists, most notably between Carston (1988, 2004b) and Recanati (1993, 2004a, 2010).

which gives rise to another dilemma:

**SD VII:** On the single-sense account, departures from the Boolean clauses for ‘and’ are explained pragmatically. The Scope Problem shows however that the content allegedly generated via pragmatic inference gets caught in the scope of the compositional operators. If so, the semanticist has to choose between *i*) granting that some pragmatic content gets caught by the Boolean connectives and *ii*) granting that the non-Boolean behaviour of ‘and’ is due to pragmatic infiltration at the semantic level. Neither option is compatible with the MDP dogma of semanticism.

In the next section, I’ll deal with the first horn of the first dilemma.

#### 4.2. Let A Thousand Senses Bloom

The problem posed by (1) and (2) is that classical Boolean conjunction introduction states that if  $\phi$  and  $\psi$  are true (and thus assertible),  $\phi \wedge \psi$  will also be true (and assertible)—whatever grounds the conjuncts on their own will also ground their conjunction.

Given commutativity,  $\psi \wedge \phi$  will also be true (and assertible) *in the very same circumstances*. Yet, intuitively, the order of the conjuncts makes a difference to the truth condition of (1) and (2) (although some may grumble that it only makes a difference to their assertibility conditions). Change the order, and you change the thought (the sense expressed by the conjunction). Moreover, as it happens, you also change its truth value, or so it seems.

These are cases, then, that seem to invalidate classical conjunction introduction (at least, if loosely taken—we’re skipping the commutative step for convenience).<sup>12</sup>

The proposal we are considering in this section denies NLB and posits that either ‘and’ is indefinitely polysemous (one sense of ‘and’ for every possible worldly relation that might hold between the conjuncts) or that we can give a precisely specified set of ‘and’-like connectives that disambiguate the various possible readings (in the case at hand, a temporal ‘and’, say:  $\wedge_{\mathcal{T}}$ , that is, ‘and then’).

<sup>12</sup>Strictly speaking, these cases pose a problem for one of the *structural rules* of a logic, namely, commutativity (and/or premise Exchange in a sequent calculus). A rigorous statement of the introduction rules would attach importance to the order in which the conjuncts are entered in the derivation but in any case the commutativity of ‘ $\cap$ ’ forces commutativity on Boolean ‘and’, given the clauses given in fn. 6.



So, on this strategy *we multiply senses*, either by listing them (we take the list to be closed and specifiable), or by taking ‘and’ to be polysemic in the same open-ended way that, say, ‘cut’ is.

Let’s call strategies of this kind MSA (Multiple Sense Accounts).<sup>13</sup>

Viewing ‘and’ as indefinitely polysemous is not, I think, something that the card-carrying semanticist could endorse (or else the compositional clauses would cease to have determinate meanings). As it happens, this is, very nearly, my own position, and I will argue for it by a *reductio* by cases.

Here, I take the only semanticist-friendly version of MSA to be the thesis that ‘and’ is multiply-ambiguous and that we can enumerate all possible *conjunct-sensitive* ‘and’-senses and state precise applicability conditions for them (note this is a form of CET: any occurrence of ‘and’ can be expanded to reach a maximally specific sense for it).

Accordingly, the MSA-analysis of (2) will be that there is a variation in truth-conditions and that we must therefore *supplement* the Boolean ‘and’ with a temporal conjunction operator that *is* sensitive to the order of the conjuncts.<sup>14</sup>

The MSA position, then, is that what ‘and’ is doing in these cases is *express* (rather than *implicate*) that ordering.<sup>15</sup>

On this horn, we quietly drop NLB, but with only minor adjustments to our semantics (we top up the minimally Boolean sense with a temporal side-kick if and when needed).

But is this the *only* way in which ‘and’ can non-Booleanly order (or

<sup>13</sup>Treating ‘and’ as ineradicably polysemic has of course grave consequences for NLB—how should we set up the compositional machinery if ‘ $\wedge$ ’ does not properly model NL conjunction? Strawson (1950: 20) made the striking claim that NL “has no exact logic”. The claim is both curiously inexact and endlessly intriguing. Is he saying it is a vague matter which logic NL has, or that it is a definite matter that it *has* a logic which is inexact? And what, exactly, is an inexact logic? Strawson (1998) is inconclusive on that, for it leaves open whether we want to say that NL logic *has* rules which are not precisely stateable, or that it has no rules at all. Strawson seems only committed to the claim that *if* there are NL rules, they do not have the character of *logical* rules, whatever *those* are.

<sup>14</sup>From at least von Wright onwards there have been several proposals to model a temporal reading of ‘and’ by an appropriate relational semantics. A good reference source is §5.12 in Humberstone (forthcoming).

<sup>15</sup>It is curious that the received version of events on these matters is that Grice rejected (or even refuted) Strawson’s view, but in point of fact Strawson (1952: 81) was already talking of ‘and’ as carrying “an *implication* of temporal order”—my emphasis. True, there was what appears a fundamental divergence of opinions, because for Strawson, but not for Grice, ‘ $\wedge$ ’ and ‘and’ differ *in meaning*. But Strawson’s view (p. 82) seems to have been that the two connectives share a common, minimal element, which, in the NL version, gets enriched by “carried” implications (which may, or may not suggest that such implications were *part of the meaning*). Were the two views really that different? For a genuine account of ‘and’ ambiguity see e.g. Link (1998: 78).

otherwise conjoin and *connect*) its conjuncts? Can we get away with just *one more sense* for ‘and’? And even before we worry about that: is there a *rule* that would fix when an occurrence of ‘and’ is temporally-laden and when it is instead minimally Boolean?

The answer to these questions is, I fear, dishearteningly negative.

#### 4.2.1. Counting Senses

On the first question, a cursory look at occurrences of ‘and’ immediately suggests that there are many, indeed quite possibly *indefinitely* many senses of ‘and’ in the market as *Ersatz* conjunction operators—‘and’, that is, can be taken to express simultaneity, locality, instrumentality, conditionality, explanatory direction, causality, and so on.<sup>16</sup>

So, once you admit the temporal reading of ‘and’ as *fully semantic* in character, you open the floodgates to an indefinitely large array of sub-(or is that super-?)senses for ‘and’.<sup>17</sup>

To fire up the imagination, just consider these examples:

(3) It’s 4pm in St Andrews and it is 5pm in Paris.

(4) Sally took out her keys and opened the door.<sup>18</sup>

In (3), it seems as if the default reading is that ‘and’ is stating that the two conjuncts describe *simultaneous* states of affairs (thus disqualifying the ‘and then’ reading)—note that the reading is forced on the hearer by knowledge about the time zone difference between Scotland and France.<sup>19</sup>

In the case of (4), instead, the sentence seems to be true *only if Sally used her keys to open the door* (giving an *instrumental* reading for ‘and’).

What these examples seem to show is that given enough patience and ingenuity it looks as if we could imagine indefinitely many dis-

<sup>16</sup>A good start on a list of examples is in Posner (1980: 186); other lists are in Carston (1988: 159, 165) and (2002: 223-24).

<sup>17</sup>Cohen (1971: 55-6) suggested a “strong sense” for ‘and’ (what I call a ‘super-sense’) that includes all possible sequential orderings for the conjuncts and gets pragmatically trimmed down in context.

<sup>18</sup>This sort of example is one of the most discussed in the literature. The (contested) intuition here is that ‘and’ does not just signal a temporal ordering of the conjuncts, but in addition is such as to make (5) true only if Sally opened the door *with her keys*. This ‘instrumental’ reading is not uncontroversial, but is plausible enough.

<sup>19</sup>Tense also plays a part of course, as we shall see later. Similarly, the context would make clear whether the speaker was offering this as a reminder or an exemplification of the time zone arrangements.

ambiguations of ‘and’, one for each connection that we can isolate as holding among the conjuncts.<sup>20</sup>

Each such reading will trigger different conditions on the truth of the conjunction: the specific ‘and’ that gets invoked will dictate that *without the relevant connection holding between the conjuncts* the conjunction will not be true.

The bare truth functionality of ‘and’ is thus questioned for these cases too; what matters is not just *whether* the second conjunct of (4) is true but rather *by what means* it came to be true—the means are part of the truth conditions.

So it seems as if, just as Grice had warned, as soon as we embark on the MSA strategy of treating ‘and’ as multiply-ambiguous (and thus of locating non-Boolean behaviour at the *semantic* level), we land ourselves with a set of ‘and’-senses that seems indefinitely extensible and thus, it seems, computationally unmanageable.

Once again, the semanticist is facing a tension between her separate insistence on two constraints, the compositional requirement (via CC) and the learnability constraint. To make ‘and’ PoC-compliant, that is, we are making its applicability conditions unlearnable. There are far too many non-Boolean ‘and’-senses for us to keep track of them *semantically*—this time CC is defeated because it seems clear that we cannot predict ‘and’s behaviour ahead of its *embedding* some novel combination of conjuncts.<sup>21</sup>

The trouble doesn’t end here though. For suppose we somehow manage to list all possible disambiguations for ‘and’ (it turned out that there weren’t as many as that nasty pragmaticist had suggested). We then face the issue of *how to state the meaning-specifications*. The problem here splits into two sub-problems:

- i) whether we can give a *rule* that fixes applicability conditions that only appeal to *linguistic* context; and
- ii) whether we can so much as *state*, for each such rule, those conditions.

<sup>20</sup> An argument of this kind can be found in Posner (1980: 188), Levinson (1983: 99), Carston (1988: §3) and (2002: 224). More in general, there are reasons to distrust a sense-enumeration strategy for *any* expression (let alone the connectives). See e.g. van Deemter (1996: 207), Bouillon and Busa (2001: xv), Taylor (2002) and Kilgarriff (2007). One reason is by now familiar: there is no way to predict *a priori* all possible senses.

<sup>21</sup> By contrast, the opposing strategy by the pragmaticist appeals to a few, easily learnable (because very basic) principles of pragmatic relevance. As was the case with CET, we have no proof that there *would* be indefinitely many ‘and’-senses, but it does seem fairly plausible (there would surely be *enough* of them to create trouble for attempts to discipline their behaviour *semantically*).

#### 4.2.2. Specifying Applicability Conditions

To see how awkward the first problem is, consider this pair of cases:

(5) Dorothy did her BA in Manchester and she did her A-levels in Dorchester.

(6) The vase is broken and it was John who knocked it off.<sup>22</sup>

We naturally read the occurrence of ‘and’ in (5) as carrying no ‘and-then’ temporal connotation, no forward-looking narrative, *only if we are familiar with the British education system*.

Speakers lacking that knowledge would be inclined to assume there is a left-to-right temporal ordering of the events described. And it seems very hard to capture the character of the defeater of the default temporal reading by purely syntactico-semantic means, since the ability to individuate the relation signalled by ‘and’ in (5) is heavily parasitic on our knowledge of specific matters of worldly fact—which lands us straight back into SD IV.<sup>23</sup>

(6) complicates matters even further, because we have a violation of the natural direction both of the temporal and of the causal relation between the conjuncts. In fact, the inversion of the natural temporal reading is mandated *by the greater plausibility of a causal connection* (the causal reading overrules the temporal one, again, it seems, because of worldly knowledge).

We could say that these are cases in which, as Carston (2002: 261, fn. 9) puts it, “general world knowledge completely reverses some alleged lexical content.”<sup>24</sup>

<sup>22</sup>Examples of this kind are given in e.g. Carston (2002: 232-33).

<sup>23</sup>There are of course skirmishes around the area: one could insist that you don’t know the meaning of ‘A-levels’ if you don’t know that they normally are a pre-requisite for a BA course. And so hearers who would mistakenly posit a temporal ‘and’ would be lacking in *linguistic* competence. However, the violation imputable to the speaker seems *much* more plausibly construed as a *pragmatic* one, rather than as the breach of a linguistic rule that might be opaque to any but the most sophisticated of hearers. A Gricean analysis seems also more plausible given the ease with which in cases of this kind we can cancel and detach each of the possible readings for ‘and’. This seems in contrast with the standard notion of semantic content as “invariant, context-independent and uncancellable” (Carston 2002: 233). I think the MSA theorist could resist this move by insisting on default rules that make semantic content cancellable by *semantically disciplined* defeaters.

<sup>24</sup>Davidson (1986: 436-37) for one notes that “it does not seem plausible that there is a strict rule fixing the occasions on which we should attach significance to the order in which conjoined sentences appear in a conjunction.” Any distinction that we make between different cases, he opines, would be grounded in skills that exceed what would count as “*linguistic* competence.” There are however familiar difficulties with attempts to keep linguistic and worldly knowledge wholly separate (see p. 54, fn. 172).

Or we could instead say that worldly knowledge *directs* appropriate sense-selection.<sup>25</sup>

Either way, I think we should concede that it seems unlikely that MSA could give us anything better than a (provisional) list of putative senses (perhaps in the form of default rules) without however being able to provide effective *sense-selection criteria* that would be based on *purely linguistic* properties of the conjuncts involved.

In fact, the process of individuating which specific non-Boolean reading is expressed by ‘and’ in a particular context seems to mobilise a very wide array of cognitive resources—and recall that MSA is a thesis about the *semantic* content of ‘and’.

But then, if we need to invoke pragmatics in order both to disambiguate from a laundry-list of meanings *as well as* to determine *when* the default rules can fire and when defeaters will instead force a switch to another sense from the list, why not let pragmatics do the job *directly and right from the start* without pointless (and computationally inefficient) detours?

Grice did have a point when he invoked Occam, then: purely semantic means won’t get the semanticist what she wants anyway. The trouble, that is, is that unlike with pure indexicals, character-level clauses for ‘and’-ambiguity would provide no *useable* guidance on occasion-sensitive applicability—they would be the idlest of semantic wheels.<sup>26</sup>

#### 4.2.3. Stating the ‘and’ meaning-specifications

Now, suppose that all of these problems can be sorted out and that we have somehow managed to compile the list of all required ‘and’-senses; by some means or other, we have even managed to identify the criteria by which applicability conditions would be regulated.

<sup>25</sup>Note that there seem to be no *syntactic* explanations available here. It’s certainly not the cleft construction of the second sentence that points to the causal reading (the causal reading would survive if we dropped ‘and’ and the cleft construction, and juxtaposed the two bare sentences). Tense is not doing work either, at least, not enough work to distinguish which reading of ‘and’ is operative.

<sup>26</sup>As is familiar, even for pure indexicals there are problems, most notably with ‘here’ and ‘now’; consider an utterance of ‘it’s cold here’: where? My office, this building, this area of town, this town, this country? On a different note, Blakemore (1992: 79) has objected to SSA accounts by arguing that ambiguity proper seems to be a *language-specific* phenomenon—lexical ambiguity, that is, is in general relative to a language: e.g. ‘bank’ is ambiguous-in-English. By contrast, putative ‘and’-ambiguity is a cross-language occurrence. I do not think this objection is at all on target, however. ‘And’-ambiguity is more properly construed as polysemic in character, and like similar much-studied cases of verb polysemy (‘open’, ‘put’, ‘drop’, and indeed ‘cut’) its cross-language occurrence is no objection.

How would we go about *specifying* these senses and these criteria in our semantic clauses, though?

The difficulty is that any meaning specification that we would give to unpack the sense of ‘and’ under consideration would typically employ ‘and’ (i.e. ‘and then’, ‘and during this time’, ‘and thereby’, etc.).

But if we take the original occurrence of ‘and’ as ambiguous, we have no reason to take its occurrence(s) in the sense-specifications as any less ambiguous—both occurrences are occurrences of a sign: why should polysemy stop on one side of the meaning-theoretic bi-conditionals?<sup>27</sup>

Once again we have a transition from an underspecified language (the object language) to its supposedly crystalline counterpart (either a homophonic metalanguage or the *loglish* mixture of much contemporary semantics) where polysemic ‘misbehaviour’ is magically suspended (at last, signs quieten down and point in one direction only).<sup>28</sup>

So, to sum up our discussion in this section: if the semanticist chooses to go the MSA way, she faces three problems.

First, admitting that ‘and’ is polysemous will make the set of its subsenses largely open-ended, for it must be able to accommodate every kinds of connections that could *ever* arise between (or among) the conjuncts.

Secondly, even if we could list all the senses, it would be impossible to specify applicability conditions in a manner that would ensure they would be fully constrained by *linguistic* means (that’s SD IV again).

Thirdly, should the semanticist succeed in overcoming the previous two problems, she would still face the fact that her semantic clauses will employ ‘and’ and thus re-open the possibility of polysemous readings for the Boolean connective at the meta-language level.

The first horn of SD VI, then, is fairly uncomfortable.

### 4.3. Keeping ‘And’ Univocal

In the previous section, the semanticist embraced a profligate meaning ontology, positing as many ‘and’ senses as there are cross-conjuncts relations.

<sup>27</sup> An objection along these lines is in Posner (1980: 187).

<sup>28</sup> Often enough, see e.g. Davidson (1967: 30), the claim is that *as long as we match ambiguity for ambiguity* across the bi-conditional all will be well. But here instead of doing that we are in fact *decoupling* OL ambiguity from ML univocality!—‘and’ means something *different* on the two sides of the bi-conditional. Lepore and Ludwig (2005: 128-29) are aware of (some) of the problems thrown up by ambiguity accounts.

In this section, Occam-like, we posit just one (semantically conveyed) sense and let pragmatic principles explain away what we now claim to be mere *assertibility* issues (felicitousness concerns aside, the conjunction will remain true as long as the conjuncts are).<sup>29</sup>

In contrast to the MSA analysis, according to the Single Sense Account (SSA) in (2) there is a mere illusion of truth-conditional variation (one that is pragmatically *implicated*, not semantically expressed). Unintuitive as it may be, a switch of the conjuncts only generates an *impression* of falsehood.<sup>30</sup> The speaker has merely violated the fourth maxim of Manner: “Be orderly”.<sup>31</sup>

On this story, then, we disqualify sentences that misdescribe the temporal ordering of the events on purely *pragmatic* grounds—uttering them does not amount to expressing an outright falsehood (a conjunction literally says that the events described by its conjuncts *happened* but is otherwise silent regarding their ordering and the connections, if any, putatively holding between them).<sup>32</sup>

The rich network of (potential and actual) connections and relations between the conjuncts is thus entirely a product of pragmatic calculations.

Now, for all the woes of MSA, the *prima facie* evidence for SSA is rather indecisive. Recall two of the main features of the classic Gricean account of conversational implicature:

- i) pragmatically inflected content is derived from semantic content via an inference that is triggered by some overt violation of conversational maxims;<sup>33</sup>
- ii) pragmatically conveyed content is cancellable.

<sup>29</sup>For Grice’s invocation of his Modified Occam Razor see his (1978: 47) and (1981: 186). Interestingly, Neale (2005: 177, fn. 23) is keen to stress that the widely-held opinion that Grice rescued semanticists from the onslaught of pragmaticists is badly off-target—Grice confided to Neale that he counted himself as a pragmat[ic]ist. Grice’s strategy could also be mobilised against Girard’s (1995: 2) ‘I bought a packet of Camels’ (presumed) counterexample to idempotency. See Frege (1923: 59, fn. 15) for a different response.

<sup>30</sup>See e.g. Levinson’s (1983: 100) summation of the view: “once pragmatic implications [...] are taken into account, the apparently radical differences between logic and natural language seem to fade away”.

<sup>31</sup>See Grice (1967b: 27). Grice (1981: 186) seems to consider it a violation of the more general supermaxim: “Be perspicuous”.

<sup>32</sup>Echoing Eric Morecambe, the Gricean could say that both conjuncts are true, although uttered not necessarily in the right order.

<sup>33</sup>In Grice (1981: 185) it is stated that an appearance of violation was necessary for (at least) some conversational implicature to arise (was Grice being compliant with his own maxims when using ‘some’ here?). Elsewhere, (1967a: 32), he also considers cases where there is no *clear* violation, but it is not obvious to me that the example he gives (the garage case) is violation-free (there’s a *jump* in the narration that needs filling in by inference).

By Grice's own lights, the cancellability test is not crucial either way.<sup>34</sup> For the cases we have examined, however, it seems fairly plausible (but *not* compelling). Uttering 'A and B' and then adding the appropriate disclaimer (e.g. "but I did not mean to say that B happened after A") seems a bit odd but not contradictory.<sup>35</sup>

The question of maxim violation is by no means settled, however. In standard cases of implicatures, without *some* violation there is no triggering of the implicatures.<sup>36</sup> And Grice (1981: 187) was adamant that the "final test" for the presence of *conversational* implicatures had to be the availability of an *explicit* (i.e. *phenomenologically salient*) derivation of it.<sup>37</sup>

Given that there appears to be no detectable violation of any of the maxims,<sup>38</sup> and thus no triggering of mandated pragmatic inference, conjunction would then have to be a case of *conventional* implicature.

But if we go that way, it then seems to me that the fundamental problem is that *there is no single type of non-Boolean conjunction* that might be conventionally triggered by pragmatics (like Strawson, Grice seems to have considered only the temporal case: we now know better than that).

An account in terms of *generalised* conversational implicatures would also face problems for exactly the same reasons—what would cause trouble for that proposal is its aspiration to full generality: the fact that so many non-Boolean conjunction-senses are (or seem to be) available seems to exclude both a fully conventionalised *and* a generalised (default-led) approach to the problem.<sup>39</sup>

<sup>34</sup>See for instance Grice (1981: 186-7).

<sup>35</sup>If 'and' is ambiguous, however, the relative ease of cancellability may be due to that very ambiguity—cancelling is equivalent to saying: I wasn't saying 'A  $\wedge$  B'; I was *just* saying 'A  $\wedge$  B'. The test in this case, just as Grice had noted more generally, is inconclusive.

<sup>36</sup>As I've already discussed in footnote 33, Grice (1967b: 32) does contemplate a group of implicatures where, he claims, there is no direct violation of a maxim. The garage case he discusses, however, is precisely a case where there is the *appearance* of a violation of the maxim of relevance. We thus derive the implicated content *on the basis of a repair strategy* that assumes the violation was only apparent. It seems to me that the distinction Grice had in mind there was between *intentional* and *apparent* violation of a maxim: correspondingly, we can have *sharp* discourse gaps (which signal overt violations of maxims) and *mild* discourse gaps (which signal compliance with the maxims). Without a *processing jolt* of either kind, however, we would not in fact be mandated *inferentially* to recover content that exceeds that strictly expressed by the linguistic material deployed.

<sup>37</sup>See also Grice (1967b: 31), García-Carpintero (2001) and Recanati (2004a: §10.3).

<sup>38</sup>As noted in Levinson (2000: 216).

<sup>39</sup>Recanati (2010: 146) too interprets Grice as being committed to a generalised conversational implicature (GCI) account of conjunction. I agree that the textual evidence (Grice 1981: 186) supports this reading. But it is also the case that the passage in question was excised in the reprint in



In the next section, we'll examine a neo-Gricean view that attempts to circumvent this problem from a pragmaticist perspective, but first I want to consider one major obstacle for the semanticist who might be hoping that the MSA account could salvage her commitment to NLB. We are finally getting, that is, to the other horn of the SD VI dilemma.

#### 4.3.1. The Cohen Objection

Leaving aside the worries I've just voiced, there is another difficulty for the SSA, one of which Grice was keenly aware: embedment of implicature-carrying sentences under the scope of the connectives seems to reveal that their (supposedly) pragmatically conveyed content is captured by the connectives.<sup>40</sup> This worry, if genuine, is fatal to the Grice-loving semanticist.

Here's the shape of the difficulty for the conjunction case. Consider the following Cohen-sentence :

(7) If Harry took off his boots and went to bed, then his mum will be happy, but if Harry went to bed and took off his boots, then his mum will be unhappy.<sup>41</sup>

It seems fairly clear that we make sense of the *contrast* between the two halves of (7) because whatever *kind* of sense it is that gets attached to 'and' does survive embedding in the conditional.

On the plausible assumption that the logical connectives only operate on semantic content, the contrast between the two antecedents can no longer be made sense of as a contrast between *pragmatically computed* content (note also that the sentences are unasserted there, so the conversational maxims would struggle to get mobilised anyway).<sup>42</sup>

Patently, the purely Boolean account of conjunction would be inadequate here, for it would predict that the two antecedents are equiva-

Grice (1989)—I suspect because Grice realised it couldn't be made to work. For the notion of GCI see Grice (1967b: 37-40), Levinson (1983: 127), Levinson (2000: 18ff.) and Recanati (2010: §2-3).

<sup>40</sup>The worry initially emerged in the case of negated conditionals: when the affirmed conditional carries an implicature, the Gricean will have to say that in negating the conditional what we are denying is the implicated content. The problem is that where there is no implicature at play, the negation of the conditional is just the standard Boolean clause (true antecedent and false consequent). See Grice (1967a: 83).

<sup>41</sup>Adapted from Cohen (1971: 58).

<sup>42</sup>There are echoes of the Frege-Geach problem here. The assumption is not just plausible: the MDP makes it mandatory for the semanticist.

lent (mum is just being *irrational*—she’s reasoning herself into a contradiction).<sup>43</sup>

But the semanticist who appeals to the Gricean story fares quite badly too. In fact, she faces the unpalatable (sub-)dilemma SD VII. For either she drops the truth-functional requirement for the embedding connective (so PoC goes out of the window) to allow conjunction to operate on implicatures too, or she drops the implicature account for the embedded one (thus admitting pragmatic intrusion into *semantic* content), both deeply damaging results for semanticism.

The difficulty, then, is that if implicatures get captured by logical operators (and the Cohen sentence seems to show that *if they exist* they *do* get so captured), then, contrary to the tenets of Griceanism (and of the anxious semanticist), implicatures must be part of semantic content, for those operators, by definition, operate truth-functionally on propositions *ahead of* (*inferentially derived*) *pragmatic input*—recall that PoC was formulated as requiring that complex meanings be a function of the *meanings* (and not of the inferentially-derived implicatures) of their constituents and their mode of combination.

It thus looks as if SSA leaves the semanticist in an even worse position than MSA. For as we just noted the second horn of the original dilemma splits into a further dilemma: either she admits that pragmatic input gets incorporated into the semantic content of the conjuncts and then processed by the higher-level compositional operations, or she relaxes the compositional requirement so that some instances of embedment will flout PoC (some pragmatic values get calculated *in parallel* with semantic ones).

Either way, the case of conjunction shows that *even with words of very-nearly logical status* it is doubtful that PoC can hold in full generality.

#### 4.3.2. The Scope Principle

The difficulties caused by embedment to an implicature account were already fully present to Grice. Twice he remarked that he had no idea how to solve the problem.<sup>44</sup> But as we have just seen the difficulties

<sup>43</sup>Just as it is unable to make sense of the other classic example from (Wilson 1975: 151): “Driving home and drinking a few beers is better than drinking a few beers and driving home”. For a semanticist treatment of this sort of case involving a Stalnaker-influenced semantics for conditionals see Stanley and King (2005: 165–66). Note that, as I have already stressed, it is essential to the notion of sense that it make us maximally rational in our competence with language. So the semanticist *must* give a story that can make sense of (7) (and of mum too).

<sup>44</sup>Grice (1989: 83, 375).

are no less severe for those semanticists who had hoped a Gricean strategy would provide a good answer to NLB.

In the next section I will consider two lines of response to this problem, a broadly neo-Gricean one and an indexicalist one. First, though, to make the problem a little more precise we need to state the Scope Principle.

**The Scope Principle (SP):** Only semantic content can fall under the scope of logical operators.<sup>45</sup>

The principle seems highly plausible and indeed constitutive of semanticism. And we have just seen the dilemma it engenders for it. There remains a question, however, as to what should follow from its application to a specific case. The principle, that is, leaves unspecified what the proper diagnosis of cases that *fail* the obvious test set by the principle should be like.

Simplifying considerably, we seem to have two options:

*i)* we could take it that there are only two choices here: either MSA is true or SSA is. SP shows that SSA fails, hence we need to revisit MSA and see if we can defend it in some other way, for the temporal ordering of the conjuncts *must* be a semantic fact (if the connectives can “catch” it), not one that could be accounted for in pragmatic terms; or *ii)* we could take SP to show that the contrast in the antecedents in (7) was wrongly traced back to the meaning (or the implicatures attached to the meaning) of ‘and’; what we missed was the possibility that it could be the *conjuncts themselves* that were carrying the additional semantic content captured by the conditional operator.

We have seen that the forced choice between MSA and a Gricean version of the SSA has landed the semanticist in another nasty-looking dilemma.<sup>46</sup>

Perhaps there is still hope for her in an updated version of SSA that

<sup>45</sup> A different formulation of the principle is in Recanati (1989: 325) and (1993: 271). Carston (2004b: 74) credits Deirdre Wilson with an earlier version and indeed she herself gives (or anticipates) pretty much Recanati’s principle in her Carston (1988: 172). Arguably, it all goes back to Cohen (1971). Recanati (2010: 147–48) has a fascinating discussion of a similar principle in the work of Ducrot and Anscombe (France’s answer to Grice). In general, the principle was intended as a test to separate implicatures from *explicatures* (a term of art introduced in Sperber and Wilson (1986/1995: 182) to denote pragmatically derived content that arises as a development of logical form, rather than as an inference *from* it). For (unconvincing) criticism of SP see García-Carpintero (2001).

<sup>46</sup> We might be tempted by another thought, however. Grice (1967a: 68) mused that, for all the apparent plenitude of meanings that ‘and’ could acquire (or merely implicate), there was a strange “kind of emptiness in the notion of conjunction”. To get a feeling for that, just go back to (5) and (6) above and replace ‘and’ by a period. Not much seems to have changed—see Posner (1980: 187), Carston (2002: §3.3), Edgington (2006: 788). So, could we go eliminativist about NL conjunction?

shifts the explanatory burden to the conjuncts. In the next section, I turn to considering this option.

#### 4.4. A Way Out? Enriching the conjuncts

Here, then, is another thought: what if it is the conjuncts, rather than the dominant connective, doing the work here? Perhaps the departures from Boolean behaviour are to be blamed on some crucial semantic content that is hidden by the surface structure of the conjuncts. Maybe that's where the elusive content that was revealed under embedment resides.

What if all that needs doing to sort out this mess is appropriately *enriching* the conjuncts so that we can bring out the various kinds of connections holding, when they do, between them?

The idea is promising. Indeed, excitingly so.<sup>47</sup> Let's see how it could be carried out. Again, and assuming the desire to preserve PoC in full generality remains in place, we seem to have two options left standing.

We could hypothesise that

- i) the semantic content of the conjuncts is pragmatically enriched either at the pre-semantic level or in parallel with it; or
- ii) the conjuncts are semantically enriched via the syntactic devices posited by LF-indexicalism.<sup>48</sup>

Let me take these options in turn.

##### 4.4.1. Enriching the Conjuncts, the Pragmaticist Way

The classic cases of conversational implicature famously fail the Scope Principle test. If you want to affirm that Jones is a good philosopher, you can't do that by denying he has a beautiful handwriting.<sup>49</sup> The im-

Well, no, because 'and' and '.' are not everywhere interchangeable. Consider unlucky Jim: (8a) Jim broke his leg. He tripped and fell. (8b) Jim broke his leg and he tripped and fell (the example is due to Herb Clark and is discussed in e.g. Carston 2002: 225). Clearly, the occurrence of 'and' in (8b) forces a temporal ordering that is left open in (8a)—the most natural (but by no means the only) reading there is one of 'backward-looking' explanation. A further reason to resist the suggestion has to do with negation, as Grice (1967a: 68) himself had noted. When we want to *deny* a certain conjunctive thought, we *need* 'and'. Juxtaposition just won't do.

<sup>47</sup>The idea is already in Quine (1950: 55-6), where he notes that temporal asymmetries introduced by the use of 'unless' may seem to make it unsuitable as a translation of the conditional. His solution is to rephrase the *components* as well. Note that this requires a modification of PoC (the content of the constituents varies under embedment).

<sup>48</sup>See §3.3.1.2.

<sup>49</sup>This is the standard story. I do not however think it is the *whole* story by any means. Consider a bunch of philosophers discussing the outcome of a recent job interview in the department. They're

plicature escapes from the clutches of negation and the semantic content of the sentence used to convey your bad opinion of poor Jones' philosophical abilities is left uncontaminated by pragmatic noise.

That's exactly what you'd expect if implicatures are genuinely post-semantic affairs that leave semantic content unaffected. Not so, as we've just seen, for the case of conjunction interpreted the SSA way. With the Cohen sentences, the traditional Gricean story cannot be made to work.

Some prominent neo-Griceans have reversed the moral normally taken to flow from the SP. Rather than taking the test to show that the stuff responsible for the Cohen effect is not implicated but is instead part of purely semantic content, some neo-Griceans use the SP verdict to claim that *some* implicatures are part of the semantic content of expressions, in this case, the conjuncts.<sup>50</sup>

GCI cases, and conventional implicatures, may be taken to show that pragmatics *can* come very close to affecting genuinely semantic content. The neo-Gricean view under consideration generalises that position: pragmatic content *does* commonly become part of semantic content.<sup>51</sup>

Now, the move is interesting, not least because it shows that the SP is forcing us towards acceptance of a *mixed* conception of PoC, i.e. a conception whereby pragmatically communicated/determined content can enter the compositional calculation of (broadly conceived) semantic values.<sup>52</sup>

There are, I think, four ways in which a pragmaticist could urge revision of PoC in the light of SP.

She could say *i)* that the sense of e.g. 'and' is pragmatically *modulated* in context: 'and', that is, is one more *ad hoc* concept that gets modified (enlarged, narrowed) in context. Clearly, the radicalness of

all seasoned Griceans. And they wonder whether it was Jones who got the job. Smith may well say "Well, I think he should get it. He's certainly got awful handwriting". Maybe I'm being nerdish, but in that context I'd read the second sentence as saying that Jones is a wonderful philosopher. This seems also to survive the Cohen test. Consider, in the same context, the following dialogue: "If Jones has a beautiful handwriting, he won't get the job." Or "If the Head of School said that Jones has a beautiful handwriting, White will get the job". If I reply "No, Jones hasn't got a beautiful handwriting", or "No, White has got better handwriting than Jones", again given the context I'd expect it to be understood that I meant to assert that Jones is a good philosopher.

<sup>50</sup>More precisely, they are part of their *compositional* values, in Recanati's (2010: 168) terminology.

<sup>51</sup>There are very large issues looming in the background here, notably the extent to which content that was once pragmatically generated gets absorbed into the grammar. See Ariel (2008) for discussion.

<sup>52</sup>Or perhaps we should join in with Putnam (1986: 292) and conclude that the pragmatic/semantic content distinction cannot be drawn *systematically*.

this proposal would attack the very heart of PoC and even the boldest of pragmaticists seem to have shied away from this position.<sup>53</sup>

A second approach would be that *ii*) first, the compositional machinery processes semantic content, and then *at the end of all the steps in the computation* the pragmatic module calculates all the implicatures for *all* the preceding stages and then infers to the overall (pragmatically-modified) communicated content of the complex expression *as a whole unit*.<sup>54</sup>

A third approach takes it that *iii*) at each compositional step where a connective rule is deployed, there is an intermediate step where the implicatures, if any, are also calculated and added to the content *before* the next stage is processed.<sup>55</sup>

Finally, we could argue that *iv*) pragmatic enrichment operates at each step in the calculation *before* the rules for the connectives are applied. In fact, on this view (often called *Truth-Conditional Pragmatics*), the compositional rules operate on *pragmatically determined* values (hybrid values incorporating both semantic and pragmatic input *in parallel*).<sup>56</sup>

Now, it is not always easy to keep track of what neo-pragmaticists are *really* committed to. I will consider here the general pragmaticist suggestion—either as a version of *ii*) or of *iv*)—that *some* pragmatically derived content gets captured by the connectives.<sup>57</sup> Specifically, that the conjuncts are pragmatically enriched and their content falls within the scope of the Boolean operators.

For the pragmaticist, then, the SP and the Cohen-sentence show an important result about NL semantics: *contra* MDP, there is pragmatic intrusion into the kind of content that a semantics is supposed to systematise, the content, that is, that is processed by the compositional machinery.

The next question is whether the conjunct-enrichment pragmaticist strategy can be made to work.

If the strategy is implemented via appeal to inference (as is done

<sup>53</sup>For all that, and as I have already indicated, this is my preferred option.

<sup>54</sup>See Sperber and Wilson (1986/1995: 257), Carston (2002: 245, 258) and (2004b: 85), Saul (2002: §3.2). These accounts differ in important ways with regard to the way in which the pragmatic calculation unfolds.

<sup>55</sup>Posner (1980: §9) and Levinson (2000) advocate a view of this kind. The resulting semantics, as Posner himself says, would be rather inelegant.

<sup>56</sup>See again fn. 28, p. 71. Recanati (2010) gives the latest version of this approach; see in particular p. 168. See also Recanati (1993: 266), Bezuidenhout (2002) and Hall (2008). Probably the best place to get a sense of the diverging pragmaticist views is Recanati (2004a: §5.3) and Carston (2004a). See also Bach (1994a, 1999b) for general criticism of pragmaticism.

<sup>57</sup>See e.g. Carston (2004b: 85) and Recanati (2010: ch. 5).

within the Relevant Theoretic framework), then the dilemma for the pragmaticist that I raised in the previous chapter will again apply: there simply is no material available for the (alleged) inference *to* the determination of semantic content to be carried out.

If the strategy appeals instead to the notion of primary pragmatic enrichment, then the problems multiply.

Very briefly, the initial difficulty is that we are not told what enrichment really *is*, we are not told how it affects surface structure, why it enriches content in a certain way rather than another, what can restrict it and prevent overgeneration (what can rule out the availability of interpretations that are intuitively precluded) and how it would interact with a compositional machinery which was designed to deal with precisely computed values.<sup>58</sup>

Furthermore, even if we grant that the notion can be made stable, we still face the problem of giving a coherent account of *how* the compositional machinery would process *mixed* values, values that are generated, *ex hypothesi*, by different faculties which seem to be very hard to integrate smoothly *within* the workings of that machinery.<sup>59</sup>

To see the broad shape of the problem, recall that the suggestion we are examining is that all the non-Boolean effects we have considered (temporal, instrumental, causal, and so on) are to be accounted for in terms of pragmatic enrichment of the conjuncts *under embedding*. When the connectives process expressions of a higher-order of complexity they will then operate on the pragmatically enriched content of the conjunction.

The question now is, how would we define the function associated with conjunction that would be responsible for the *compositional* processing of pragmatic enrichment?

Formally speaking, compositionality is a stepwise upwards compu-

<sup>58</sup>Recanati (2010) discusses objections along these lines and makes some defensive moves. I think the difficulty he faces is that of attempting the (near-impossible) task of integrating pragmatic processes that by their very nature appear to defy systematicity into a fully systematic framework that perfectly parallels the truth-conditional semantics one. All too often, the clauses Recanati ends up proposing have a *whatever-it-takes* flavour. See e.g. Recanati (2010: 9), where we are told that there *is* a function that, given an expression in context, returns as a value a particular sense modulation function that is “contextually salient/relevant/appropriate” at that context. Why think that we need a *function* to do *that*? Talk of idle wheels comes yet again to mind.

<sup>59</sup>The pragmaticist’s problem here is not merely terminological: she needs to *accept* a *robust* distinction between semantic and pragmatic values for her position to be distinctive—indeed, for her to be able to *state* it and counter the semanticist’s claim that only semantic values are properly said to be part of the compositional machinery. In doing so, she is forced to say that there is *semantic* content (e.g. the expanded material in the conjuncts) that is *pragmatically* generated, which in turn gives rise to the technical problems of how to properly *define* the meaning function for conjunction that I discuss in the text.

tation that generates new semantic values from *previously determined* values for lower-complexity items.

In contrast, the account under consideration makes the enrichment of the conjuncts occur *as the conjuncts are embedded* (that is, as their semantic values *are being* calculated).<sup>60</sup>

If so, it is the operator itself that has to determine the content of its arguments (there is no properly determined *compositional* content assigned to them *until* embedment).

And we have indeed seen that every problematic example throws up different cross-conjuncts connections (and hence different enrichments) which can only be established *after* the conjuncts have been embedded.

It thus looks as if the compositional machinery will need to make four recursive calls on the conjunction operator.

First, the bare conjuncts are Booleanly embedded, that is, their truth value is processed; that, *ex hypothesi*, is however as yet insufficient to determine the truth value of the whole. So far so good.

At the second step in the computation, things already get muddled.<sup>61</sup> For it seems as if the conjunction operator will have to pass on the bare embedded conjuncts to the pragmatic module, so that their content can be pragmatically parsed to discover any temporal/causal/instrumental connection that may exist.

Then, *and only then*, will the conjunction operator be instructed by the pragmatic module appropriately to enrich the conjuncts' unembedded content *and* to order them accordingly.

Finally the operator will re-parse the enriched content of the conjuncts *as a conjunctive thought*, assign it a truth-value and then pass it on to any embedding operators, so that *they* can operate on the fully enriched content.

The same four-stage procedure will then be replicated at each further step in the meaning computation for expressions of higher complexity, if any.

Clearly, at the very least this looks horrendously inelegant. But I'd also argue that it is incoherent: no function could be coherently defined that would perform these calls. Here's why.

It is at step two that the account gets into trouble and incoherence

<sup>60</sup>TCP thus requires a form of *co-compositionality* of the kind suggested in e.g. Pustejovsky (1995) that I discussed in fn. 145 on p. 47. In that framework, however, the semantic values for the arguments of the function are pre-defined ahead of embedment, whereas in TCP they are generated more or less *ex nihilo*.

<sup>61</sup>We do not have any detailed story on the table, so here I'm trying to second-guess the pragmatist. As Kadmon (2001: vi) has noted, for the most part we only have "hopelessly underdeveloped sketches of pragmatic accounts".



threatens, for the examples we have considered so far clearly show that the ordering of the conjuncts is something that *reflects* rather than *determine* the cross-conjuncts relation.

In processing those examples, that is, *we make retrospective sense of the ordering because of the cross-conjuncts relations we detect* (by some cognitive means or other) *as holding independently of the ordering*.

The natural thing to say then is that it is the facts as reflected in *un-embedded* occurrences of the conjuncts that should determine which kind of enrichment is required for the conjuncts.<sup>62</sup>

If we instead make enrichment *functionally dependent* on embedding, we force the meaning function to backtrack half-way through the computation and then forever *loop*.<sup>63</sup>

The loopy incoherence here is due to the fact that we are asking the conjunction operator to do two incompatible things: *determining* the enrichment of the conjuncts as a pragmatic function of their ordering and *detecting* the required ordering as a function of the enriched content; both ordering and enrichment, however, remain undetermined *until* embedding.<sup>64</sup>

I thus conclude that the idea that there could be *compositionally-disciplined* pragmatic enrichment is not coherent.<sup>65</sup>

#### 4.4.2. Enriching the Conjuncts, the LF-Indexicalist Way

Perhaps ungenerously, I have given the pragmaticist suggestion rather short shrift. What of its semanticist counterpart?

If we now go back to the SP from a semanticist perspective, we could read it as providing grist to the LF-indexicalism mill, since the Cohen sentence can be used as a test to discourage the very idea that pragmatic chaff could make a contribution to *semantic* content (as we have just seen, the resulting semantics for complex meanings would be impossibly unwieldy and almost certainly incoherent).<sup>66</sup>

<sup>62</sup>In the Harry case: whether one should say e.g. ‘Harry went to bed and took off his boots afterwards’ or ‘Harry went to bed and he *had taken off* his boots *before doing so*’.

<sup>63</sup>The need for processing backtracking is not *per se* peculiar to TCP accounts. Partee and Rooth (1983: 353, fn. 11) discuss garden-path cases arising from conjunctions mixing extensional and intensional verbs (in that order) causing difficulties for their type-lifting account.

<sup>64</sup>Obviously, the detour via the pragmatic module won’t help the conceptual hurdle here.

<sup>65</sup>In essence, the problem for TCP is a special case of the dilemma for pragmaticism I discussed back in §3.5.

<sup>66</sup>LFI and TCP do coincide in their assumption that the compositional machinery can be made to work; they diverge on the question of what does the enrichment (syntax in one case, free pragmatic enrichment for TCP). See e.g. Recanati (2010: 92-3). On the other hand, as we shall see, LFI has a point of contact with Carston (1988: 161) too, who holds that the temporal ordering of the

It can then seem as if in response to the SP we are left with no alternative but to posit additional *semantic* material to account for the non-Boolean phenomena which have been puzzling us.

Recall that we got into this fix because initially we tried to account for an apparent divergence in the behaviour of ‘and’ from that of ‘ $\wedge$ ’ by positing a pragmatically derived e.g. temporal sense for ‘and’, superimposed, as it were, on the Boolean core.

The embedding test provided by SP can however be taken to show that it was a mistake to try and attribute the divergence from the Boolean ‘and’ to *pragmatic* factors (at least: to pragmatic facts attaching to ‘and’ itself).

The lesson should instead be: we did not uncover enough *semantic* stuff beneath the surface structure of the *conjuncts*, or so argues the LFI theorist.

As I discussed already in chapter 3, LFI posits a variety of hidden variables nested at LF which get assigned values in context. The proposal in the case of e.g. (2) is that the variables in question do a kind of temporal book-keeping job.

LFI assumes (following a suggestion in Partee 1984b) that there is a broad, pragmatically enforced maxim regulating discourse, namely that a sequence of sentences frames the narration in a forward manner.<sup>67</sup>

This broad maxim forces semantics to look at the syntax, where the temporal record of the succession of the events described is stored as the conjuncts unfold.<sup>68</sup>

With this in mind, LFI can be seen as making two main claims with regard to ‘and’: *i*) that the conjuncts are *semantically* enriched in a way that is fully controlled by the syntax (the enrichment obtains at LF level); *ii*) that the enrichment is relative to a context.<sup>69</sup>

These two claims, I will argue, land LFI into trouble, both with regard to the descriptive adequacy of their account of conjunction and with respect to their more general commitments to PoC and MDP.

events is a “by-product of the reference assignment process” (for the events/times described in the conjuncts).

<sup>67</sup>As we have seen in the case of (5) and (6) above, this maxim is defeasible. One immediate problem for LFI is that the variety of ways in which it can be defeated risks trivialising a syntax-based proposal.

<sup>68</sup>This is the picture in Stanley and King (2005: §V), see in particular their fn. 38 on page 164.

<sup>69</sup>Both claims can be found in Stanley and King (2005: 164).

## 4.4.2.1. Syntax and Logical Form

The first LFI claim is explicitly empirical: at the level of syntax, each finite verb is said to head a tensed verb phrase with a hidden adjunct that specifies the time of occurrence for the event described.<sup>70</sup>

LF-indexicalists repeatedly appeal to “best” syntactic theory (indeed, often enough, their argument is one from authority). Syntactic theory, however, keeps changing the amount (and character) of the structure it posits.

The particularly acute problem for LFI is that in the current framework of the Chomskyan Minimalist Programme (CMP) (and presumably LFI theorists would consider the Chomskyan orthodoxy as what is best in the field) there is no room left for LF.<sup>71</sup>

More generally, the LFI conception of logical form owes much more to the logician’s view of that notion than to the linguist’s—for even when Chomskyans were still talking about LF, their notion wasn’t quite as robust (and semantics-friendly) as LFI assumes.<sup>72</sup>

Furthermore, the notion of variables that LFI employs has *no place at all* in current linguistic theory.<sup>73</sup> The entities recognised by syntactic theories are those that can be shown to play a part in syntactic *derivation*. LFI variables, however, only appear, if at all, *after* the derivation has been carried out and their only motivation is seman-

<sup>70</sup>After making rather precise proposals, Stanley and King (2005: 164, fn. 39; 165, fn. 41) then declare themselves to be “agnostic” as to the precise nature of the (syntactic) mechanisms that implement their LFI with respect to these cases. The point is: given the claimed *empirical* status of these elements, this does not quite seem the right position for them to take. But in general, and as picked up by Collins (2007: 807, 816, 826, 838, 841), LFI *defers* to the best current syntactic theory, *whatever it is, and whatever structures it posits*.

<sup>71</sup>Despite the peremptory Chomsky quote at the opening of Collins (2007) and further statements by Collins himself (e.g. p. 811), this statement is neither fair nor entirely right, for LF survives in early Chomskyan minimalism (Chomsky 1995a,b), only to be discarded in very recent work, e.g. Chomsky (2006: 16). It however survives in other accounts, see e.g. Lasnik and Uriagereka (2005: 8) and Boeckx (2008b: 44-5). What gets jettisoned outright is the idea that LF might reside at *Deep Structure* and with it the misleading contrast between Surface and Deep Structure. But even that is not as clear-cut as it might seem. See Uriagereka (2008) for a very recent defence of D-Structure from *within* the minimalist program. It is however correct that the notion of LF standardly appealed to in philosophy of language (and Stanley and King are no exception to this) profoundly differs from that used in linguistics. For some discussion (and contrast) see May (1985), Hornstein (1995) and Preyer and Peter (2002).

<sup>72</sup>Collins (2007) again has a sustained argument in that respect. Even a cursory glance at the linguistics literature will confirm that the problems raised by Collins are robustly supported by the evidence. Of course, there is no reason to think the linguists’ view of LF is the *right* one, but as I noted already LFI routinely appeal (and ostensibly defer) to their *authority*.

<sup>73</sup>As remarked again by Collins (2007: 832).

tical (if at all).<sup>74</sup>

And the guiding principle in syntactic theory is that any posited structure must be traceable to a specific step in the derivation. No such step exists for the LFI variables.

So it seems as if the two crucial moving parts in the LFI machinery (LF and hidden variables) receive little or no support from the CMP.

In a nutshell, the methodological problem for LFI is that the main selling point of the theory is its adherence to syntactic constraints; the LFI machinery, however, has no *syntactic* motivation at all (indeed, it lacks any support by the syntactic *evidence*). This seems bad enough already.<sup>75</sup>

#### 4.4.2.2. Context-relative Semantic Enrichment

There is, to my mind at least, an even more serious problem facing LF-indexicalism. For suppose there *was* enough structure at the syntax level to carry the information required to express the needed connections holding between the conjuncts (maybe the CMP will be re-inflated again at some point in the future and LFI variables, and indeed LF itself, will be found a place in the syntactic pantheon).<sup>76</sup>

Suppose then that the LFI theoretical machinery is in place and does enjoy the support of best syntactic theory. I contend that LFI would still face a raft of problems, as I hope to show in this subsection.

Granted, the LFI account *does* solve the Cohen problem, for the two conjuncts are now properly distinguished and mum doesn't turn out to be irrational after all.

One major problem remains, however, and it is that LFI invalidates conjunction-elimination. Let me try to explain why I think this is the case.

Since Stanley and King (2005) do not give a fully worked out semantics for 'and' but only a bunch of sketchy remarks, there may be some room for manoeuvre against the argument I'm now assembling, but here's what they *seem* to be saying.

<sup>74</sup>The point is repeatedly made in Collins (2007: 831). I think it is, as matters stand, unimpeachable.

<sup>75</sup>There is an additional worry, which I'll discuss later. While LFI seems to handle the temporal case pretty smoothly (up to a point, as we shall see shortly), when it comes to the other readings for 'and' (causal, resultative and so forth), Stanley and King (2005) have to resort to pragmatics again, taking their clue from Saul (2002). But as we depart from the temporal model, where one simple, straightforward maxim seems sufficient, we require increasingly more complicated and case-specific maxims and their ability to *directly* affect syntax seems less and less plausible.

<sup>76</sup>This has happened before. See also the brief discussion in fn. 71.

According to LFI, a sentence like (2), relative to a context, expresses a proposition of the form:<sup>77</sup>

(2b) He took off his boots at time  $t$  and went to bed at time  $t + n$

The reason why (2) expresses this particular proposition, and not e.g. :

(2c) He took off his boots at time  $t + n$  and went to bed at time  $t$

is that there is “a pragmatic maxim that affects semantic content by influencing the semantic content of temporal elements *in the syntax*”.<sup>78</sup>

Note however that once the context is fixed the temporal information in the conjuncts is, as it were, given *absolutely* (i.e. the value for  $t$  is kept constant).<sup>79</sup>

My claim is: this picture is deeply problematic for a semanticist (and again, deeply incoherent). To see why, consider the quote that I’ve just given. We are told that the pragmatic maxim digs directly into semantic content “by influencing” the temporal elements in the syntax.

But now take the context as we have fixed it with this interpretation for (2). In that context, given how the pragmatic maxim has “influenced” the syntax, the following will be true, and fixedly so:

(C1) He took off his boots at time  $t$

(C2) He went to bed at time  $t + n$

If the temporal indexing is part of *what the conjuncts semantically express* (that’s the main LFI contention, isn’t it), then, once the content-in-context of the conjuncts has been fixed, reversing the order in (2) should cause no problem at all: *they should still say the same thing in that context*, no matter how you order them (*those* were the boots-bed facts *as described by syntactic elements*, after all).<sup>80</sup>

Once pragmatics has directed *syntax* to provide a specific semantic enrichment, then, the *hidden* layer of content thus determined will remain fixed, no matter what happens at surface structure.

<sup>77</sup>I’m not entirely sure what notion of proposition is at play here. It seems to me they are thinking in very fine-grained terms, possibly along the lines detailed in King (2007) (and one of its sources of inspiration, Richard 1990).

<sup>78</sup>Stanley and King (2005: 164, fn. 38), my emphasis. As I’ve pointed out before, the maxim is derived from Partee (1984b: 254).

<sup>79</sup>It is of course CET at work again here.

<sup>80</sup>Note that, assuming the picture of propositions at play in Stanley and King (2005) is ancestrally derived from Richard (1990) (via the essays now collected in King 2007), (2) and (2d) *say the same thing* at a context and so do e.g. (C1) and (C1b).

In particular, the LFI claim that the *conjunction* in question, relative to *that* context, expresses the proposition given in (2b) cannot be seen as *blocking* a double Boolean step of conjunction-elimination (giving us (C1) and (C2)—we are, after all, trying to secure a way for the semanticist to keep ‘and’ Boolean.

But if the temporal information is normally *hidden*, we could re-write the conjuncts as:

(C1b) He took off his boots

(C2b) He went to bed

And now, again given LFI’s Boolean allegiances, there is nothing that can prevent a conjunction-introduction step to:

(2d) He went to bed and took off his boots

(2d) however comes out intuitively false relative to the original context (that was precisely the problem we started from), and yet there is nothing in the LFI account that can stop the ‘and’-elimination + ‘and’-introduction steps. On the contrary.

The problem, exactly as for the TCP theorist, is that we are making the conjunction operator, rather than the context, responsible for the enrichment of the conjuncts. By doing so, however, we end up treating conjunction as *an indissoluble entity*.<sup>81</sup>

It follows that if we accept the LFI account, there can be no backward road from a conjunction to its conjuncts: LFI may solve the SP but at the cost of invalidating conjunction-elimination.<sup>82</sup>

There are further problems for LFI, though.

First, their claim is that pragmatics affects semantic content by priming syntactic elements in the conjuncts. But now reflect that it is perfectly possible to *cancel* the e.g. temporal sequencing of events as stated in a conjunction. If LFI gives the correct story about conjunction, however, we could never, even jokingly, mis-describe the facts and *then* specify their appropriate ordering, for that ordering is *ex hypothesi* taking place *at the level of the syntax* (we have *literally* said the wrong thing, not just pragmatically implicated it as in the original Grice story). Indeed, the very act of cancelling would be *unintelligible*.<sup>83</sup>

<sup>81</sup>Indeed, we are treating conjunction as relevantist fusion, for LFI now has to say that ‘and’-elimination steps are invalid.

<sup>82</sup>Were the LFI theorist to reply that I am misunderstanding their proposal, and that the enrichment only takes place under embedment, I would respond that this too would invalidate conjunction-elimination.

<sup>83</sup>To repeat, the LFI claim is not that a defeasible maxim forces a pragmatic *inference*. It is, rather,

Secondly, LFI faces the same problem I raised with regard to the pragmaticist proposal in a previous sub-section. Under LFI, *semantic* enrichment depends on pragmatic factors in a way that would invalidate standard views of PoC (and MDP), for either the LFI picture is that *i)* you can't *yet* enrich the conjuncts *in a certain way* until you are *directed* to do so by pragmatic assumptions *that only arise under embedment* or that *ii)* the *syntax-led* enrichment of the unembedded conjuncts will not be fixed *even within the same extra-linguistic context*. Either way, PoC fails.<sup>84</sup>

Thirdly, in the case of e.g. instrumental readings LFI would have to posit adjuncts attached to the VPs (e.g. in the case of (4): 'opened with her keys'), and the problems we now face are that

*i)* there is nothing given in the syntax of the conjunct itself that demands the adjunct (if anything, the adjuncted material would be quasi-anaphorically recovered from the preceding conjunct in (4), thus defeating direct, i.e. local, compositionality);<sup>85</sup>

*ii)* the number of such potential/optional adjuncts would have to be as large as there are cases of instrumental/causal and countless cognate connections and their insertion would depend on discourse relations that seem hard to trace back to the syntax.<sup>86</sup>

The paradox then is that *because of* their reliance on syntax, LFI theorists are thus committed to an even more radical form of pragmaticism than their rivals, for their strategy amounts to a claim that pragmatics intrudes *not just* on semantic content *but on the syntax as*

that a maxim *brings out* content which is *already* represented at the level of syntax.

<sup>84</sup>I'm here excluding a retreat strategy for LFI where the compositional rules for 'and' are changed so that *the syntactic content* of conjuncts changes under embedment. Apart from defeating the NLB claim, this is tantamount to a form of truth-conditional pragmatics. In Stanley and King's (2005) terminology, weak pragmatic effects are controlled by the standing meaning of an expression (paradigm case: the assignment of reference to indexical expressions); strong pragmatic effects aren't. LFI is firmly committed to the denial that there are any strong pragmatic effects on semantic content, including that of the compositional rules (Stanley and King 2005: 140) and (Stanley 2000: 395). However, *for LFI to work at all* no determination of the needed *syntactic* adjustments can be made *until* the discourse relations and the relevant worldly knowledge have been fully processed.

<sup>85</sup>Direct compositionality requires that syntax and semantics work very closely (i.e. locally): at each step in the syntactic derivation, the semantic module is *already* constructing an interpretation. The LFI account of conjunction is non-local, in the sense that we cannot assign an interpretation to the conjuncts until all the (pragmatically regulated) discourse relations (including potential defeaters) have been assessed. LFI cannot therefore implement direct versions of PoC. Whether that is a threat to PoC as a whole is a different issue of course. See the essays in Barker and Jacobson (2007) for more on this topic.

<sup>86</sup>Note that in e.g. (8b), fn. 46, p. 115, the causal direction between the tripping/falling/leg-breaking events is hardly likely to be traceable to *syntax*. A semantics given in terms of events such as those in Parsons (1990) and Pietroski (2005a) could perhaps help.

*well* (a claim not even the wildest of pragmaticists would make!).

This paradoxical result is a consequence of the fact that according to the LFI semantics for ‘and’ the conjuncts cannot be enriched *until all the relevant discourse features have been identified* (i.e. not just the relative position of the conjuncts in the conjunction, but also their interaction with past and future discourse).

It then seems to follow that the conjuncts need to pass on information to each other *over* the dominant connective (up the derivation tree rather than along sister nodes, that is) for the relevant connection to become salient *and able to “influence” syntax*.

And we can now see another point of tension between LFI and current syntactical thinking. As it happens, one of the few principles to survive relatively unscathed from the Principles & Parameters era into the CMP is the Locality Principle which dictates that grammatical operations be local<sup>87</sup>—and indeed, the principle’s resilience under substantial theoretical revision suggests its crucial importance.

Under the principle, the relevant connections would have to be traced to the *closest* element in the derivation, and the connective is the obvious place where to look.

Positing a *syntactic* interaction *across* conjuncts where extra-clausal distance is involved seems thus to violate one of the key CMP assumptions (LFI *syntactic* conjuncts enrichment could not *possibly* be local, that is).<sup>88</sup> Once again, the putative support for the LFI position by the ‘best syntactic theory’ seems to evaporate.

And breach of locality is not the only worry here. For what controls phenomena of this kind—e.g. the reverse temporal ordering in (5)—seems to be largely dependent on worldly information (once again, there seems little prospect of a *general* account that covers all cases ahead of embedment).

But for many linguists, and certainly for those working within the CMP, *syntax* does not have to concern itself with worldly knowledge (*that*, they claim, is a matter for a theory of performance, and not of competence).<sup>89</sup>

As we have seen, LFI makes much of its deference to syntactic theory and if so, it goes against the grain in making syntactic facts (the emergence of optional adjuncts not required at any stage in the

<sup>87</sup>See e.g. Radford (2004: 15).

<sup>88</sup>In fact, the LFI picture here faces the same problems I raised for the truth-conditional pragmatics approach, in that the conjunction operator would have to call on the conjuncts back and forth a couple of times before directing their enrichment as a function of their content *and* of any discourse relations that might be discovered holding between (or among) them.

<sup>89</sup>I’ve questioned this claim repeatedly but it’s a semanticist theory we are testing now.



derivation for the finished sentence to count as grammatical) depend crucially on worldly knowledge.

In short, if rhetorical dependencies of the kind we have been considering are *grammatical* dependencies, then they have got to respect the Locality Principle and that seems to suggest that if we are still looking for a *semantical* solution to the problem, then MSA (of some form or other) is the way to go, given that conjunct-enrichment violates that principle. And so the semanticist who went the LFI way is now thrown back to the first horn of SD VI.<sup>90</sup>

To sum up, the LF-indexicalist framework invalidates conjunction-elimination and can only preserve PoC for ‘and’ by going *against* a vast amount of syntactic evidence. And by doing so, it breaks, without *any* syntactic motivation at all, the rule-to-rule component of PoC, one of the main tenets of the compositional credo.<sup>91</sup>

#### 4.4.2.3. Patching LFI: Conjunction As Deletion

Now, if I am right, LFI as it stands does not provide a satisfactory solution to the problems that have been exercising us in this chapter.

I have indicated that my own view of the matter is that the difficulties we have encountered in trying to match ‘and’ to its Boolean counterpart point towards the conclusion that *even logical words suffer from indeterminacy*.

Before closing, however, I want to offer the semanticist a way to patch the LFI account of conjunction.

My suggestion is simple: we should think of the classic Gricean pragmatic maxims as regulating *the deletion of syntactic material* in the conjuncts.

As on the standard LFI account, the conjuncts are semantically enriched at a context according to syntactic constraints, but on my proposal the enrichment is *a) mandated by the facts as described* and *b)*

<sup>90</sup>Curiously enough, at times Stanley and King (2005: 164, 176) seem indeed to be endorsing an MSA-like view whereby the default pragmatic assumption of orderliness *extracts* from the temporal/causal information contained in the conjuncts a *pragmatically* derived non-Boolean sense (‘and then’, ‘and as a result’). This suggests the sketchiness of their remarks regarding ‘and’ may indicate some indecision on their part. Even this solution, however, would require profound changes to the compositional machinery, because ‘and’ (that most ubiquitous of connectives) will have not only to be made sensitive to those elusive hidden variables but also e.g. to return the value ‘false’ when the conjuncts are presented in the wrong order (and with that, the truth-functionality of ‘and’, and not just its commutative character, is completely lost, for it no longer takes as arguments truth-values only).

<sup>91</sup>We would that is have an *artificial* increase to syntactic structure so as to shadow the additional semantic material needed to keep track of discourse relations. The situation here is analogous to the one I discussed in chapter 2, fn. 124, p. 41.

it occurs *prior to embedding*. Indeed it occurs *whether or not* the conjuncts get embedded.

Some uses of ‘and’ (and indeed *most* unembedded uses) will sanction deletion of syntactic material for general reasons of conversational economy; indeed, what material can be *safely* deleted (elided) will *always* be a question of pragmatic maxims of various kind (chiefly: the optimal informativeness requirement attached to the Maxim of Quantity).

Here’s a plausible default rule: if the salient relation between the conjuncts is sufficiently perspicuous or sufficiently entrenched in use, delete any material that would redundantly specify it!<sup>92</sup>

NL conjunction would thus be kept strictly truth-functional, PoC would be fully preserved and the problems that I have been considering above would, I think, disappear.

This is so because on my proposal what pragmatics controls is not syntactic *enrichment* (which is instead kept fixed in a context), but rather *deletion* of syntactic material that is determined and generated by the facts-at-a-context.<sup>93</sup> Syntax is thus kept fully insulated from pragmatics, just as the LFI account intended but did not quite ensure.

The explanation of the *infelicity* of (2d) in a context where the facts are as described by (C1) and (C2) is given by the fact that *informationally essential* syntactic material was unduly deleted from the conjuncts.

A similar explanation would be available for the Cohen sentence (mum wasn’t irrational, because *different material* got deleted in the two conjunctions).

Cancellability too can be explained as backtracking after *having deleted too much*—the deletion failed the optimality test.<sup>94</sup>

<sup>92</sup>For example, in (4) pragmatics would normally force the elision of the ‘with the key’ adjunct from the second conjunct. The maxim would nicely comply with the anti-redundancy methodology of CMP. Chomsky has always maintained that ellipsis is deletion, so I would expect this proposal to be fully consistent with the spirit of CMP.

<sup>93</sup>This is in effect a kind of reversal of the proposal sketched in Edgington (2006: 789) and a modified version of the one given in Cohen (1971) where a single rich sense for ‘and’ had been posited, with non-salient senses removed/neutralised in context. I think Edgington’s proposal suffers from the same issues as LFI. Cohen’s suggestion is good but he wrongly locates the disambiguating stuff in the semantic content of ‘and’ rather than the conjuncts, as well as making the sense specification too vague (‘and’ expresses “some kind of relation” holding between the conjuncts). That requires, among other things, that the meaning of ‘and’ be both non-Boolean (or super-Boolean) and continuously modulated as new ways to exploit rhetorical discourse relations are devised. On my proposal ‘and’ remains firmly Boolean and we need posit neither new senses nor new syntactic material.

<sup>94</sup>Yes, I know. I’m defending a view that depends on the falsity of –CET from the previous chapter, for to say that a deletion is optimal is to say that it was maximally specific and I’ve given plenty of reasons to doubt such a notion is tenable. My lifeline to LFI of course assumes the pragmati-

It seems to me that the fact that syntactic material is *deleted* lends plausibility to the claim that ‘and’ remains Boolean, for on my proposal what pragmatics regulates is deletion of *pre-existing* syntactic material and not its *enrichment* under embedment and so its role remains limited to the processing of *post-semantic* content, just as Grice had intended.<sup>95</sup>

Indeed, in the case of the SP test, we can argue that the connectives operate *on what the content should be under optimal deletion*; they *assume* optimality constraints have been respected and process content accordingly.

The phenomena under scrutiny here, then, are best seen as cases of *ellipsis*, a well-studied *syntactic* phenomenon, which should please LFI-inclined theorists.<sup>96</sup>

#### 4.5. Conclusion

Do we now have an answer to the NLB, then? Well, it seems to me that the revised LFI framework that I sketched above has got some claim to have solved the problem in favour of a positive answer to NLB.

The snag of course is that the LFI patch depends on the truth of CET, a thesis which I have done my best to invalidate in the previous chapter.

On the pragmaticist side, by contrast, the NLB-reply seems by and large negative: the TCP account will require of ‘and’ that it be made sensitive to the calculation of indefinitely many pragmatic values, while the Relevance Theory view is that the NL connectives are not really truth-functional at all (they do not operate on fully propositional content but only on schematic entities).<sup>97</sup>

cist worries about CET can be resisted. Some of what I say here also seems to be inconsistent with the difficulties I raised back on page 126—the range of cases and intra-conjuncts relations are too numerous to be captured by a set of rules. But, to repeat, my proposal is that in a context each conjunct would carry enough syntactic material to support a minimally non-ambiguous representation (hence, we are not even entering CET territory here). Pragmatic maxims entirely to do with ease of *performance* determine what can be safely deleted to minimise discourse-relative communicative redundancy. My account should thus parry the worries in section 4.4.2.2 because the material is *already there anyway*.

<sup>95</sup>Commutativity is obviously preserved, because the deletion of the material needed to communicate the actual order of events won’t be licensed in the “wrong” case.

<sup>96</sup>One would of course need an account that tallies with current syntactic thinking, but that does not seem an impossible task to achieve. I lack the space to sketch out a fuller proposal, but I hope the direction of further research in this area to make the proposal fully testable is sufficiently clear.

<sup>97</sup>To be frank, I’m not at all clear what the RT view is with respect to the clauses for the NL

I have however indicated my reasons for distrusting, once again, both semanticism and pragmaticism.<sup>98</sup>

Given my doubts regarding CET, I would argue that the only surviving option is the rather bold (if not wild) idea that the NL connectives are as polysemous as the non-logical vocabulary of our vernacular.<sup>99</sup>

In the next chapter I consider the implications of this conclusion as we turn to examining the CLB thesis.

connectives. As I've said already, their view is that a truth calculus can only apply to sentences in the language of thought and not to NL—see e.g. Carston (2002: 257). I've expressed my doubts about the tenability of such a view in the previous chapter. The connectives are supposed to operate on truth values, and yet on the RT account there is no truth value to be fed to them. Despite that, we are somehow supposed to derive from the composition of lower level schematic entities a higher-level schematic entity on which we can then perform Gricean inferences to the explicated content of the whole by reaching out, *through the connectives*, as it were, back to each of the constituent schematic entities. Quite frankly, I think this picture is deeply confused.

<sup>98</sup>I haven't considered dynamic accounts of conjunction. The dynamic view of conjunction is anticipated in Stalnaker (1970) and Stalnaker (1974: 60): first assert P and then assert Q. There are several implementations of the dynamic framework in the literature (see Muskens *et al.* (1997) for an overview). I will only note a couple of things. First, the dynamic account of conjunction stumbles on a major problem: it flattens out the distinction between juxtaposition and conjunction (as noted in Elbourne 2005: 15). Secondly, it forces a radical revision of the notion of consequence, for dynamic conjunction will typically fail not just Commutativity but Reflexivity too. It also, as it stands, invalidates PoC.

<sup>99</sup>I have repeatedly described my view as radically minimalist about content. My claim about polysemy might therefore seem inconsistent with that commitment. Well, the fact is that once again we are trading on an ambiguity (!) between two different conceptions of meanings: minimalist semantic content is polysemous when viewed from the semanticist perspective; it is firmly (disquotationally) univocal when viewed from the minimalist perspective itself.



## Chapter 5

### Situating Consequence

[A] sentence to which formal logic is applied must be thought of as making one fixed statement and no other.

Quine (1953b: 145)

[O]ur interest, as theorists of meaning, is in specifications of the *contents* of actual and possible speech acts—specifications of the thoughts propounded in them. [...] [S]pecifications of the contents of assertions will *be* specifications of truth-conditions for the sentences used to effect those assertions.

McDowell (1987: 89)

Work in logic just is, to a large extent, a struggle with the logical defects of language, and yet language remains for us an indispensable tool.

Frege (1915: 252)

How strange if logic were concerned with an ‘ideal’ language, and not with *ours*.

Wittgenstein (1930: I, 3, 52)

#### 5.1. Introduction

Over the course of the previous chapters we have been confronted with an abundance of ‘logical defects’ in our NL. Complex meanings seem underdetermined by the lexicon, expressions refuse to stay still and their much-needed (by the theorist!) univocality seems impossible to secure; even the sentential connectives appear to display indeterminacy features.

If all the problems I have raised are genuine, what prospects for logic then? As the batch of quotes above shows, it is standard practice

to assume that logic needs determinate pieces of content before it can attempt to discipline our reasoning.

Even the weakest of semanticist assumptions—that all that semantics does is study the systematic connections between assignments of semantic values to expressions—is under pressure, for in the absence of a *general* guarantee that we have a firm grip on what determinacy of content comes to, we lack a guarantee that the crucial dividing line between logical and non-logical vocabulary has been correctly drawn, and hence that the compositional machinery has been appropriately primed.

In this chapter, then, our topic is the impact of the essential instability of the sign on our cherished view of logic. Here's the plan.

In section 2 I argue that the claim that logic is the science of reasoning is in tension with some of the key theses of semanticism discussed in previous chapters. In section 3 I defend the view that logical revision is content-driven and that Lewis' attempt to resist anti-CET worries by appeal to the notion of uniform disambiguation does not succeed. In section 4 I dismiss idealising strategies standardly invoked in defence of LSR. I conclude by introducing the notion of situated inference in section 5, before concluding that the only way to salvage LSR is to espouse a radically minimalist conception of content.

## 5.2. The Science of Reasoning

Logic, every textbook tells us, is the science of reasoning.<sup>1</sup> Call this claim 'LSR'. My task in this chapter is to test LSR. I will argue that it is incompatible with some key semanticist theses (above all, MDP).

Specifically, LSR, as it stands, makes two claims (that logic has universal applicability and that it is purely formal) which cannot be reconciled as long as standard semanticist assumptions are in place. Assuming semanticism, that is, if there *is* a relation of genuinely logical—i.e. *content*-neutral—consequence, then that relation is empty (the more precise content-determination becomes, the less general that relation).

<sup>1</sup>From Shoenfield (1967: 1) and Lemmon (1965: 4-5) to Tourlakis (2003: 1), Hedman (2004: xii) and Restall (2005: 1). Not just textbooks. See for instance Shoesmith and Smiley (1978: x) and van Benthem (1995: 271). Dummett (1981: 15) puts the idea in the clearest terms: "logic is that theory which is required for the analysis of deductive reasoning in general. Within particular regions of discourse, distinguished by subject-matter, there may be forms of inference peculiar to those regions, and they will not be the concern of logic as such: but logic must incorporate all principles of inference that may need to be invoked independently of subject-matter." See also Russell (1914/1993: 66-67). Smiley (1982-1983: 3ff.) begs to differ.

LSR can be taken in two ways, descriptively or normatively. My concerns so far have been with (linguistic and reasoning) competence. Hence I'll ignore the first reading and just take LSR to be the claim that logic studies how we *ought* to reason.<sup>2</sup>

Now, the logic of the textbook is almost invariably classical logic, the logic, that is, of *indefeasible* reasoning (as Frege insisted, a calculus of *truths*), whereas our epistemic predicament is rather that of taking our (quasi-)deductive chances from a constitutively *defeasible* information base to equally defeasible conclusions.

One might therefore be tempted to suggest that the logic of *our* reasoning ought to be a dynamic, non-monotonic logic of *belief* and *belief revision*, rather than a static calculus of (unattainable) truths.<sup>3</sup>

I think there is much to be said for a position of this sort. CL condemns us to modes of reasoning—e.g. those sanctioned by the truth-conditions for material implication—that are intuitively invalid, and it leaves out much that we ought to systematise, if what we are after is a full account of the distinctive ways of rationality (the missing bits are, roughly, those I briefly sketched above).<sup>4</sup>

It would thus seem that precisely to the extent that the classical paradigm of logical thinking is a *heavily* idealised one (it models, if anything, unerring mathematicians reasoning about non-time-bound entities), it can serve no purpose as a guide to those reasoning modes that do in fact characterise us as rational (removing the friction of the rough ground doeth not make for *better* modelling

<sup>2</sup>I'm being very quick with this but it seems clear that any argument in favour of the competence/performance distinction in linguistics will apply, *mutatis mutandis*, to logic. Nevertheless, as I'll suggest later, if our logic of choice fails *too badly* at the descriptive task, we may have reason to question its normative import. Similarly, it is no coincidence that those authors who, like van Benthem (1995: 271), insist that logic *ought* to be engaged in description of actual reasoning practices should have a keen interest in *alternative* logics.

<sup>3</sup>One complaint along these lines (but at the service of a different technical approach) is in Devlin (1991: 10) where classical logic is declared totally unsuited to the claim of being *the* overarching logic presiding over our practices precisely on these grounds. There are now well-established accounts of the logic of subaltern reasoning and dynamic inference. See e.g. van Benthem (1996), Rott (2001), Bochman (2005), Makinson (2005) and van Ditmarsch *et al.* (2008). Lewis (1982: 103–05) has an intriguing response to keep CL in place: belief sets are fragmented and inference is allowed only with respect to *unmixed* premises within consistent subsets. The idea is striking (we only ever reason using consistent fragments of an inconsistent corpus). It still fudges the issue though. For example, *which* fragment was Frege working with prior to Russell's letter? We cannot simply *stipulate* it was consistent (it most certainly included Basic Law V).

<sup>4</sup>Defeasible reasoning is just a variant of suppositional and modal reasoning more generally, and these are reasoning modes that are clearly absolutely central to our thinking (normativity, and rationality, are essentially modal notions). On these matters, however, there is hardly a consensus regarding what principles ought to go into *the* logic of the textbook (i.e. which account of the conditional, which system of modal logic and so forth).



tools in the case of reasoning: it makes for modelling of *different* phenomena altogether—precisely, those inhabited by the unerring mathematician).<sup>5</sup>

My interest here, however, is not with arguing for a particular thesis about *the* logic (or the privileged class of logics) that better qualifies as the (fabled) *one true logic* (or family thereof).

What I want to test, rather, is the general claim that *some* logic(s) *could* be the science of reasoning, regardless of *which* specific logic(s) would be put forward as *the* logic(s) of choice. The difficulty I raise is highly general, and if genuine it will apply whatever the details underpinning a particular formulation of LSR.

### 5.2.1. The Generality Claim

I take it that the generality claim, the claim that logic is the most general of all sciences, is a defining part of LSR (logic is the science of *all* reasoning).<sup>6</sup>

In the preface to *Begriffsschrift*—the work that on the story standardly told inaugurated the era of modern logic—Frege defined logic as what *transcends* all particulars (its generality derives from its formality, that is, from its *disregard* for content).<sup>7</sup>

Presumably, transcendence cannot and mustn't mean that logic doesn't *apply* to those particulars, though.<sup>8</sup> Rather, logic has to be *both* a *calculus ratiocinator* and a *lingua characterica* (i.e. a formal calculus that covers both formal reasoning and *thinking* more generally).<sup>9</sup>

And it seems to me that those of us who place a special value on the

<sup>5</sup>I should point out in passing that I think CL is a bad model even of mathematical reasoning. We might however think with Harman (1999: 28, 46) that logic merely lays down specifications for what counts as a proof, without having to chart the ways in which proof connects to reasoning. If logic were silent on those connections, however, why should proof exert any rational force on us?

<sup>6</sup>Indeed, often enough textbooks will introduce mathematical logic as the study of mathematical reasoning—e.g. Shoenfield (1967: 1), Wolf (2005: vii). Note also that the motivation behind many (perhaps most) deviant, 'heretic' logics is that the proposed replacement is better than CL when it comes to accounting for our *everyday* reasoning, see e.g. Mares (2004: 3-4), Brady (2006: 5), Routley *et al.* (1982: xi). In contrast, defenders of classical logic will typically appeal not just to theoretical simplicity, but to CL's *indispensability* for some aspects of our practice, e.g. mathematics (see e.g. Burgess 2009: ch. 5), over the supposed (local) advantages of rival logics.

<sup>7</sup>On the notion of formality see e.g. the discussion in Beall and Restall (2006: §2.5).

<sup>8</sup>Indeed, in *Grundlagen*, p. iii, Frege makes it clear that "[t]hought is in essentials the same everywhere: it is not true that there are different kinds of laws of thought to suit the different kinds of objects thought about." This is why I disagree with Sher's (2000: 113) contention that Frege's *Begriffsschrift* was conceived as a purely logical rather than a universal language.

<sup>9</sup>Frege (1897b: 242).

role of logic in our practices will also have to insist that formality *and* universality (of applicability) are the necessary marks for something to qualify as *the* logic that governs our thinking. It is *that* combination of claims that is *the* reason why logic has the unique normative hold on our practices that it does enjoy.<sup>10</sup>

Remove the universality claim, and it becomes obscure why logic should be anything more than a specialist tool for reasoning that is local to highly specific areas of discourse. These two claims, then, are the defining marks of logic, and yet they are irreconcilable with the truth of semanticism, as I will try to show in the sequel.

Now, it is of particular interest to my concerns that on the Fregean conception the whole point of logic is to make *precise* the “*expression of a content*”—where content for him was, in modern parlance, *inferential potential*.

On this (pretty standard) view, logic can achieve its overall goals thanks to its (purported) ability to eliminate guesswork from NL so that *fully determinate* thoughts can be expressed by means of symbols and thought-transitions disciplined accordingly.<sup>11</sup>

Indeed, Frege insisted that the greatest advance over Boole that he had accomplished was a better analysis of the *internal* structure of propositions—i.e. modern logic gives us finer cutting tools for the crucial task of carving out content to codify proper inference.<sup>12</sup>

Another way of looking at this is to say that *until and unless* we have settled on the appropriate way to individuate content (i.e. inferential potential), we do not possess warrant to claim that our logic is doing its job in the required way.

Similarly, if we want to preserve the claim that logic is the most general of all sciences and the one that encapsulates the laws of thinking, we must be careful not to idealise away too much.

Generality, that is, requires the unrestricted *applicability* of the peculiarly logical modes of inference that our textbooks codify—the inferences that are sanctioned by the textbook must be those that *we* (and not some impossibly idealised agents) would have to respect.<sup>13</sup>

I shall return to this point a couple of sections down the line.

For now, let me expand a little on the assumption (or claim) tacitly made by LSR that, appearances to the contrary, normal everyday

<sup>10</sup>I think the standard view is *de facto* committed (or somehow equivalent) to Fregean universalism about logic. The discussion in Goldfarb (2010: 68-9) is useful.

<sup>11</sup>See e.g. the discussion at Frege (1880/81: 11-13).

<sup>12</sup>Frege (1880/81: 14-5).

<sup>13</sup>The abstract and general features of reality that logic studies (Russell 1919/1993: 169), that is, must be features of *our* reality, not that of some unseen gods.

reasoning *can* be appropriately regimented and made fit for testing against the standards set by logic.

#### 5.2.1.1. CET and the Structure of Proof

This, in effect, is a CET-equivalent claim applied to the notions of proof and inference. It amounts to saying that behind the haze of use logical thinking *is* instantiated and it is logic (of course!) that dictates its essential features.<sup>14</sup>

All that needs doing to vindicate (sound) everyday reasoning, then, is to fill out the various gaps that economy of conversation leaves open and then show that the properly expanded pieces of content *and* properly expanded pieces of reasoning conform to those principles that we have identified at the formal level.<sup>15</sup>

So, just as the surface structure of NL sentences is generally *defective* to the extent that in the absence of *further* specifications it leaves its content underdetermined, so is the surface structure of inference (we reason by leaps and bounds, but the gaps, on the CET view, can *always* be filled appropriately).<sup>16</sup>

Accordingly, CET does double duty as the essential tool in our efforts fully to capture the formal (inferential) properties of thoughts (as the senses of eternal sentences) while also allowing us to capture the equally precise structure of proof and inference (as appropriately disciplined movement between precisely specified thoughts).

The CET-equivalent claim with respect to proof (and reasoning in general) is thus that any piece of informal reasoning can be transformed into an extensionally equivalent formalisation where no step is omitted and all transitions are shown to be in accordance (or otherwise) with the basic laws of logic.

Properly seen, then, CET is not just a technical claim about NL semantics: it also encases an ideal of rationality as *theoretical reason* (whatever reasoning move we make is translatable into a fully explicit, rigorously assessable form; whatever piece of content we entertain

<sup>14</sup>There are times when it seems as if Frege is saying that only thinking that accords with logical laws *is* thinking properly speaking (i.e. a *truth-directed* activity), see e.g. Frege (1893/1998: xv) and (1897b: 128, 149).

<sup>15</sup>A claim of this sort is explicitly mooted in e.g. Priest (2006: 198).

<sup>16</sup>Indeed, right from the start Frege made clear that his great technical achievement, namely, the making explicit of the hitherto glossed-over structure of proof, was also a profoundly *epistemic* one of huge *normative* import, for it traced the ultimate justification for our privileged forms of reasoning back to *a priori* laws of logic that determine what counts as properly rational thinking, indeed as thinking at all.

can be made fully univocal).<sup>17</sup>

In fact, the two roles of CET cannot be torn asunder: a conception of content as the fully determinate inferential potential of the thoughts we standardly entertain (implemented by the notion of eternal, fully de-contextualised sentences assigned absolute truth values by the semantics) goes hand-in-hand with a conception of inference as codified by precisely stipulated relations holding between eternal sentences.

Let me assemble these reflections into one principle:

**Determinacy Thesis for Logic (DETL):**  $LSR \Rightarrow CET$

What the thesis says is that any logic that is Fregean in scope, that is, any genuinely *universal* logic, requires pieces of determinate content to operate upon. The threat here of course is contraposition—without determinacy for the relata of the consequence relation (CR) (*and* for the logical constants embodying its properties), we cannot systematise reasoning.<sup>18</sup>

If CET is unimplementable, then the claims of logic become *at best* conditional in character: *should* we succeed in making content determinate, then the CR will have a field to operate on; otherwise it will be empty.

Let me now recall two claims I made in earlier chapters. I argued that CET *is* unimplementable and that the logical constants too display indeterminacy features (or rather: that attempts to make their content determinate end up in dead alleys).

If so, DETL fails. There are no candidate contentful items that we can take our non-logical *and* logical vocabulary to range over. And if content has to be fully determinate, then unless underspecificity is

<sup>17</sup>Let me anticipate my sympathy for the view of rationality sketched in Ryle (1962) and Hacking (1983). On that view, CET is wrong on both counts. More about this in chapter 6.

<sup>18</sup>As remarked in Black (1937: 77). Let me wave away the classic objection (raised by Hartry Field in conversation) that asks why we should worry about determinacy of content for the *substituents* of the non-logical constants. The very notion of topic-neutrality presupposes that nothing but the meaning of the logical constants will determine whether an inference counts as genuinely logical. Indeed, we can use *obscure* pieces of content to *exemplify* logicity—classic case: the move from ‘Every tove is slithy’ and ‘Alice is not slithy’ to ‘Alice is not a tove’ (Bell and Machover 1977: 5). As long as interpretation is kept fixed across different tokenings, we are told, everything is all right. Reply: the point I am considering is whether we can so much as make sense of what it means *to keep interpretation fixed synchronically*, let alone diachronically (for worries about *that*, see e.g. Williams 2008: §II). Similarly, it is not clear that we can prevent the indeterminacy worries from extending to what we normally class as logical vocabulary. If indeterminacy-as-instability is an essential feature of the sign, why should logical *signs* be immune to it?

banished we can entertain no content at all.<sup>19</sup>

The upshot is that we have to give up on LSR altogether and restrict the scope of logic to mathematics and the study of the properties of logical systems at a purely formal level.

Logic would thus turn out to be merely a highly specialised tool used in meta-reasoning of a restricted kind, rather than the most general of disciplines.

Note here the connection with the discussion of NS back in chapter 2: to neutralise worries about nonsense, semantics has to assume that meaningfulness questions have been settled before it gets started; similarly, logic will have to assume that underspecificity questions have been settled before it can get going.<sup>20</sup>

Uncertainty on both points makes both semantics and logic conditional enterprises—*something* else does the dirty job for them, and their claim to full generality is thus severely curtailed. Not quite the picture the standard view requires, I'd say.<sup>21</sup>

### 5.3. Content, Validity and Logical Revision

Now, there is an obvious line of resistance to my argument above. I seem to be arguing for the (confused) conclusion that considerations to do with *content* could falsify claims about a discipline that has traditionally prided itself on its *disdain* for content. Wasn't logic supposed to transcend all particulars, and haven't I just quoted that Fregean remark myself?

Well, yes, but things are not that simple.

Let me try to further motivate my insistence on the pivotal role of CET with a few, very basic examples from the textbook and from the history of logical revision.

I have already quoted Frege's claim that his logic was superior to Boole's because of its better discriminatory powers with respect to inference.

<sup>19</sup>DETL is most readily associated with Frege's work of course. I think we should be reluctant to think it is an easily discarded thesis however. Frege was remarkably forthright in confronting the issue head on. Most writers after him have for the most part ducked the issue. Or, if discussing it at all, have set it aside with a shrug of the shoulder (see e.g. Lewis 1982: §3). Russell (1923) is one (partial) exception: but even he ducks out at the last minute, appealing to some sort of Humean naturalism. Williamson (1994: ch. 2, 6, 7) tackles the issue head on, and so does his (2000). As far as I can tell, Williamson's position amounts to a sophisticated version of LIR (see §5.4). To that extent it does suffer from the problems I discuss later in the text.

<sup>20</sup>The requirement is most explicit in Lewis (1982: 107). Strikingly, his discussion in the following two pages concedes that there is no guarantee CET can be made to work.

<sup>21</sup>In this respect, the first three sections of Black (1937) hit the nail square on the head.

For my purposes, it is crucial to note that Frege articulated his claim along three lines: *i*) that his logic had greater *expressive* powers; *ii*) that his logic could account for a *larger* class of intuitively valid patterns of inference; and *iii*) that his logic achieved the dual (and simultaneous) task of codifying inference *and* concept-formation processes (whereas Boolean and Aristotelian logic had to *presuppose* concepts were already formed *in a particular way* and in good standing).<sup>22</sup>

It is received wisdom that Frege's work represented the greatest advance in the history of modern logic.<sup>23</sup> But it was an advance triggered by (and implemented through) considerations to do with *content* (through that advance, a better grasp of content was to be had).

Crucially, content-individuation and codification of inference are co-occurring acts; and I am urging that we cannot separate worries about content from claims about logicality.<sup>24</sup>

Let's see why.

According to semanticism, the logical constants are re-absorbed into the form of the expressions they govern. What counts as *the* logical form of an expression, however, is clearly a function of our best conception of content in force at a time.<sup>25</sup>

Defenders of modal logic, for instance, have claimed that Frege (1979: §4) missed a trick in dismissing modal content as belonging merely to a theory of force because by doing so he overlooked the effect of modality on inferential potential and hence on content.<sup>26</sup>

There is therefore a very close connection between content-individuation and the range of inferences we are interested in defining as logical—quite clearly, the proper description of the dispute here is

<sup>22</sup>See the already quoted Frege (1880/81: 14-5). Frege speaks here of concept-formation but elsewhere (1897b: 127) stresses that we grasp, rather than generate, thoughts. I take 'formation' to refer to the process of *modelling* concept-formation in our logic.

<sup>23</sup>Well, that it is received seems undoubted. That it is wisdom has been questioned by e.g. Putnam (1982) and Boolos (1994).

<sup>24</sup>See e.g. Frege (1882: 75/55; 74/53), where the role of logic is given precisely as that of splitting content into its "ultimate components", thus showing "the manifold logical relations that join thoughts together". Sullivan (2004: 696) has worried about this aspect in Frege's conception of logic, since it seems impossible to settle inferential properties independently of (and in advance of) settling questions of identity of content.

<sup>25</sup>I am not claiming that from the undoubted fact that our conception of logicality is indexed to our conception of content it follows that logicality is an *intrinsically* relative question. In fact I am not sure we can even make sense of the *absolute/relative* contrast in this (or in any other) case. I am simply arguing for the point that logical revision (and the attendant re-drawing of the logical/non-logical distinction) is a content-driven process. That's why CET matters to the issue. Burge (1998: 354) is useful in this respect (the determination relation between logical structure and the study of inference runs both ways).

<sup>26</sup>See e.g. Fitting and Mendelsohn (1998: 5).

that Frege thought modal operators do not express a specific content but merely flag the speaker-relative degree of epistemic entrenchment with respect to a given proposition; advocates of modal logic by contrast think there is such a thing as modal content and (or rather: because) it *does* affect inference in ways that rightfully qualify as logical.<sup>27</sup>

This sort of dispute concerns which (possibly contentious) schemata we ought to incorporate in our logic. But consider also a very basic example familiar from the textbook involving perfectly uncontroversial schemata:  $p \vdash p \vee q$ , we are informed, is either an instance of the schema  $\varphi \vdash \psi$  or of  $\varphi \vdash \varphi \vee \psi$ .

We are then told that only the latter is the appropriate schema under which to subsume sentences of that form and the moral is supposed to be that we need to uncover *just as much* internal *structure* as is required to give a plausible account of validity (the first schema would be insensitive to a logical particle in the conclusion which ensures validity: content was carved too coarsely under that schematisation).<sup>28</sup>

Quite. But given existing controversies with regard to the validity of certain contentious schemata, I maintain that it is still an open question whether we have cracked this particular nut (the one relating to the appropriate connections that have to obtain between schemata and their instantiations).<sup>29</sup>

Consider, that is, live issues such as the relevantist logician's attack

<sup>27</sup>One might object that this problem affects only Fregean views of content, where content individuation is given in inferential terms. I deny this is the case at all. Any account of content individuation will suffer from the same conceptual stumbling block, for logical inference will always be defined in the same terms as those chosen for giving identity criteria for content. We *always* draw the logical/non-logical demarcation in the very act of choosing identity criteria—as a brief examination of accounts given in terms of concept possession, acceptance conditions or even plain truth-conditions will show.

<sup>28</sup>Often enough, this sort of example is used to introduce students to the transition between propositional and predicate logic (i.e. from a coarse to a finer-cutting logic).

<sup>29</sup>Here's yet another problem: logic's main claim to *practical* utility is its (purported) ability to make *confusing* NL arguments *clearer* by bringing out their *structure*—see e.g. Kalish and Montague (1964: 14) and Forster (2003: 3–4). Since there seems to be no way of isolating structurally important aspects of content independently of our views of logicity, it seems as if this task cannot be carried out either. This is so because the individuation of the logical components of those aspects *cannot* be neutral with respect to our intuitions regarding what *should* count as a valid argument. Logic's ability to clarify the structure of argumentation, that is, essentially depends on our (pre-existing) judgements regarding validity: only *after* we have settled on a view regarding logicity can we then use the tools thus individuated to clarify muddled arguments. The paradox then is that we are supposed to establish what counts as valid reasoning by making manifest what is hidden behind the confusing cloak of NL expressions (and the haze of use), but we can't do that until we have demarcated the logical/non-logical divide, which in turn presupposes we have *already* clarified the muddle those tools were supposed to dispel.

on the paradoxes of implication and McGee's purported counterexamples to *modus ponens* reasoning.

In the latter case, much of the controversy is over whether McGee's cases are genuine *instances* of the MPP schema. The claim, that is, is that there are *hidden bits of content* that once made manifest do show that everything is all right (the defender of MPP thinks it inconceivable that MPP could fail: hence a better account of *content* is proposed to salvage the logicality of the schema).<sup>30</sup>

In the case of the relevantist critique, the proposal is that the rules for the conditional (and other operators) need to be *restricted*. But again, the relevantist's broad requirement of variable-sharing is first and foremost one to do with the *content* of expressions (we need to cut them more finely than on the classical view: we need to represent *more* of their content in the logical form that regulates that particular inferential step; and this time the finer account of content that is proposed is at the service of *downsizing* the class of validities).<sup>31</sup>

I could easily give more examples and in greater detail, but I trust that the general point is fairly clear.

Faced with questions about validity thrown up by controversial examples, logicians will either insist that we cut the content of the propositions involved more finely or that we revise our class of validities so as to incorporate fresh patterns determined by a new view about the content of (or rather: about the form/content demarcation for) the propositions involved.<sup>32</sup>

<sup>30</sup>Sometimes the claims about missing content are not intended as a defence of MPP. See e.g. Lycan (2001: 64). Sometimes they are (even when ostensibly about context-switches and assertibility), as in Gauker (2005: 86-7). In general, the discussion of McGee conditionals shows that *philosophers standardly assume that we can pack an awful lot into content*. MPP is salvaged, that is, by showing that acceptance of a certain "bare" sentence always involves acceptance of a complex set of beliefs somehow attached to the content of the sentence—see for instance the discussion in Bennett (2003: §61).

<sup>31</sup>For the requirement see e.g. Brady (2006: 4ff.). For an unusual take on relevance see Lewis (1988). The discussion in Burgess (2009: ch. 5) is instructive. Burgess first notes that relevantistic logic (as he calls it) insists there be *topic overlap* across an inferential step for it to be valid. Using Craig Interpolation Theorem, he easily shows that even the classical logician can ensure (trivial) topic-overlap *in all but the degenerate cases* (the ones involving the paradoxes of material implication). Burgess then concludes that because of the indispensability to mathematical practice of those degenerate cases there is no reason to insist on the topic-overlap requirement in full generality. So, we have here a *pragmatic* explanation (indispensability) for the insistence that our logic need not discriminate content as finely as the relevantist logician wants.

<sup>32</sup>One more example, vagueness. There have been recent attempts (e.g. Cobreros *et al.* 2010 and Zardini 2008) to impose local restrictions on some structural features of the CR, e.g. transitivity, to deal with vague terms. Yet, these attempts face the problem that unless we *can* discriminate vague terms from their precise counterparts, if any, there is no way to implement the proposal: before we can disambiguate the CR we need to have settled whether or not the pieces of content concerned



To sum up: logic may indeed be defined as a topic-neutral discipline. That claim, however, is one we can legitimately make *only when taking logic as normal science*.<sup>33</sup>

Whenever pressure is applied on the precise shape of its laws, logic must (and typically does) pay heed to content. In short, *logical paradigm-shifts are content-driven*, they are shifts in the ways we take ourselves to be grasping content, in the ways we model our grasp of content (indeed, in the way we read UaGS).<sup>34</sup>

Logical revision, then, is first and foremost revision of the tools we use to individuate content (each time we move bits of our vocabulary across the non-logical/logical divide, each time we impose or lift restrictions on existing logical particles, we are thereby changing not just the fine-grainedness of our logical tools but also our conception of the content they operate upon).<sup>35</sup>

If CET cannot be implemented, however, I do not think we can be as confident as we seem to be that the standard account is telling the whole story about logic (how can we legitimately draw the logical/non-logical divide in the absence of an assurance that we have individuated the content of the propositions involved precisely enough?).

Before I can move on, there is an immediate objection to the argument of this section which I need to address.

One could argue that all that logic is required to do is identify, for each piece of content, *initial segments* of their CET expansion.<sup>36</sup>

It will then be a matter of deriving general logical principles from the appropriately logical patterns that emerge from examination of those segments. As long as we can agree on the character and structure of the initial segments it doesn't much matter whether or not we

are vague. A further problem concerns the *ad hoc* nature of the restrictions on transitive chains (3 elements for Zardini, 2 for Cobreros *et al.*). Why not 4 or 3? Or 6 or 243? And how can we tell if we can get away with chaining a hundred pieces of reasoning (for by their own admission transitivity is not *always* bad)? These, it seems to me, are insuperable problems. No formal approach will ever give a theory-independent answer to these questions.

<sup>33</sup>In Kuhn's (1962) sense.

<sup>34</sup>If LSR is to hold, the content it oversees must be made up of the subjects of our attitudes. We may argue about the needed level of transparency, but reasoning is moving between evidence-based beliefs, and hence *some* degree of transparency is needed. ).

<sup>35</sup>Logicity is often defined as invariance under permutation. Wouldn't that escape my CET-based argument? Well, as is familiar, this proposal helps give necessary conditions for logicity but it over-generates. Casanovas (2007) is a good recent discussion (with references to the relevant literature). As far as I can tell, at present there is no definitive result in this field that could secure a precise and uncontroversial demarcation of the logical/non-logical divide.

<sup>36</sup>A suggestion of this kind can be found in Russell (1957: 121-22).

can *complete* the expansion process,

The objection has initial plausibility, but if one goes back to my discussion of ‘and’ in the preceding chapter, it should be clear that we have reason to doubt that we could individuate even the initial segments with the sort of precision assumed by the logician (for the worries we raised there concerned precisely the individuation of the *initial* fragment; the questions before us were: is there *such a thing* as a temporal/instrumental/causal conjunction operator? is *it* properly classified as *logical*?—and those questions arose well before doubts about CET-*completion* would ensue).<sup>37</sup>

The fact is, we test validity by appeal to examples stated in NL. Counterexamples are formulated in NL. Relevant facts about contextual embedment and its impact on assertibility are spelled out by clauses given in NL.

If CET is a chimera, so is the idea of precision even with respect to initial segments of the expansion process.

In case you are still unconvinced, let me say a little more.

### 5.3.1. Disambiguation and Inference

I have been arguing that the act of marking out the division between the logical and the non-logical is *eo ipso* the act of individuating certain pieces of content as the pieces they *are* by virtue of their inferential properties (or acceptance/possession conditions, or contribution to truth-conditional content). We do both things at the same time. The two acts are conceptually inseparable.

The difficulty was: how can we establish that the logical vocabulary is appropriately insensitive to the *particularities* of content if we cannot individuate content *semantically* in the first place (especially: not in a logic-independent manner)?

Here’s a strategy to deal with the problem, suggested in Lewis (1982: §3): suppose we abandon the notion of truth-preservation *simpliciter* and adopt a notion of *truth-preservation on some disambiguation* of the sentences involved. We assume (or rather: pretend) that CET can be implemented, and implemented uniformly.<sup>38</sup> We then check for truth-values distributions under (potential) disambiguation. Problem sorted.

<sup>37</sup>Indeed, those doubts *specifically* concerned whether any *further* steps in the expansion process were needed (e.g. from ‘and then’ to ‘and then as a result’, and so forth).

<sup>38</sup>I’m aware of no-one who has given a convincing proof that CET *can* be implemented—and the close of Lewis (1982) is remarkably (and disarmingly) indecisive in that respect: no such proof is available; we just shrug our shoulders and move on.

Well, I don't think so. This response, I contend, would be far too quick and in fact question-begging.

Firstly, Lewis (1982: 108) simply stipulates that we *can* reach a stage in the disambiguating process where we have settled on a *range* of truth-values assignments under disambiguation. But that is precisely what the anti-CET objection challenges and it cannot be assumed without argument (and neither Lewis nor the original Quinian strategy of eternalisation have provided one).<sup>39</sup>

More importantly, the Lewis view assumes that we can make sense of *sameness of disambiguation*, a notion that we require in order to enforce the principle of uniform substitution.

But the anti-CET proponent will challenge that assumption too, for whether there can be *non-syntactic* criteria for what can count as the *same* sentence is precisely the point in dispute.

Given the CET worries, why should *a* given stage in the expansion process qualify as expressing the *same* disambiguation across an inference if what supposedly does the disambiguation is just more expressions?

If we need to *start* the expansion process in the first place, we are conceding that signs are unstable (that syntax alone cannot disambiguate); but then it's obscure why we should take *any* stage in the completion process to be any *less* unstable.

The thing is, sameness of disambiguation is a deeply incoherent notion. And the difficulty is as stark as it is simple to state. It is conceded on all hands that *sameness of syntactic structure does not mean sameness of interpretation* (or disambiguation)—that's what it *means* for there to be ambiguous expressions in a language.

But if so, we can make *no sense* of the notion of sameness of disambiguation either, for at all stages in the disambiguation process we'll have a given syntactic structure that will be just as open to multiple interpretations as the first stage. Syntactically, the expansion process has merely added more complexity. In absolute terms, the expression is still ambiguous. And there is nothing that can fix its interpretation univocally, and hence nothing that can fix disambiguation univocally. It follows that the idea that we could *compare* disambiguation steps is conceptually incoherent. Lewis' strategy cannot succeed.

<sup>39</sup>Quine (1950: 56) defended the thesis that as long as interpretation is *uniform* within a reasoning context, there is no problem at all. In other words, as long as CET worries are spread equally across the board, schematicity is unaffected (but again, note that the very choice of words reveals the instability of the view: *uniformity* presupposes we can make independent and antecedent sense of the notion of *sameness of form* and of the form-content separation—exactly the issues at hand).

#### 5.4. Logic and Competence: Against Idealisation

I have been arguing that LSR makes two jointly unsatisfiable claims, for logic cannot fulfil its universalist ambitions by disregarding questions of content (by holding on to uncompromising topic-neutrality, that is). The CET-worries raised in chapter 3 are in fact amplified in the case of logic because of its need to carry out two tasks at once (individuating content and codifying inference) which in fact presuppose each other. Without an answer to CET, however, neither task can be carried out. Or so I have argued.

Let me now consider a typical move by the defender of LSR, namely, the further claim that logic idealises reasoning (LIR) and that therefore the issue of a poor match between ‘natural’ and logical reasoning is no objection to the account.

Ideal reasoners, the proposal goes, would have no difficulty in securing determinacy (presumably because *they* would have access to a magical language). Odd as it may sound, *our* logic tracks *their* behaviour. And *we* are rational to the extent that we approximate *their* ideal standards of reasoning, precisely those encased in the logical laws (indeed, without that regulative ideal to aspire to, our reasoning practice would be a wholly random affair).

LIR, in effect, is a more radical form of CET. The claim now is that every reasoning step we take in our daily reasoning *could* always be translated into reasoning steps between truths *as expressed and grasped by ideal reasoners*. Correct reasoning is reasoning in conformity to the logical laws stated by and meant for such reasoners.<sup>40</sup>

Clearly LIR does not (yet) settle the issue of which logic is *the* logic of reasoning. Indeed, as I’ll be arguing shortly, if we espouse LIR we do face the problem of saying why *the particular idealisation we have chosen* should be *the* one that we have to best approximate in our everyday reasoning—for unless we can have an *independent* hold on the question (which seems doubtful), it is an arbitrary matter at which point we want to say that idealisation is *misrepresenting* our practices, given the proposed choice of ideal logic and the reasoning modes that it commends to our attention (the question, that is, is what *independent* reasons can we find in support of the selection of a specific level of logical strength for our logic of choice).<sup>41</sup>

LIR, however, is often invoked to defend the claim that CL is the

<sup>40</sup> A similar thesis is commonly held in epistemology with regard to the notion of epistemically ideal agents. See the recent Christensen (2007) for discussion.

<sup>41</sup> Again, McDowell’s (1981a: 342) remark about the fantasy of a viewpoint external to our practices applies here.

one true logic, that it is not just the logic of science and mathematics, but also the logic that best regulates our (properly idealised) daily affairs.<sup>42</sup> So let me say something more specific about that claim.

One obvious reply here is that it seems unclear why we should accept CL as mandated by LIR when it endorses inferences (e.g. the implication paradoxes and monotonic reasoning more generally) that *by the lights of that practice* ought to be patently invalid, while also failing to account for inferences that again any reasonable account of that practice would classify as intuitively valid (e.g. various forms of meaning entailment that are no less logical than those attached to the traditional connectives and so on).<sup>43</sup> Which prompts the question: *why* insist on a LIR strategy that falsely represents what our reasoning is really like both procedurally and extensionally?<sup>44</sup>

Here's a counter-reply the LSR/LIR defender could give: my critique of her position is assuming that reasoning modes and the requirements of ideal rationality must be fully *user-transparent*.<sup>45</sup> This assumption however is unrealistic: ideal rationality is a regulative ideal and may well elude our grasp in a given context (where, for instance, we may be overwhelmed by pragmatic noise in our reasoning).

Reasoning, the reply continues, is often a case of doing the best one can in the circumstances, and our cognitive limitations should not impact on what the regulative ideal is like.<sup>46</sup> Conversely, the bad arguments (e.g. positive paradoxes of implication) are only *pragmatically* fallacious. True, they would be neither persuasive nor *practical*.

<sup>42</sup> Actually, and as already indicated, I think CL is the wrong logic for science and mathematics too and not just for their folk versions. It may be the most *convenient* proof-theoretically speaking, but *that* does nothing to establish the claim that it is *true* of those domains.

<sup>43</sup> Read (2004: §5) defends a wider conception of validity and of the scope of logic: logic is the study of valid inference, and logical validity is only a sub-class of validity. Read (1988) gives a mild-mannered attack on the LIR-motivated defence of CL. Anderson and Belnap (1975: 5) and Routley *et al.* (1982: xi) are a little less diplomatic.

<sup>44</sup> Wittgenstein (1945/1953: §426) insists that a logic that would be unattainable in actual practice would be an idle wheel of dubious normative hold. I imagine LIR-defenders such as Williamson will remain unperturbed. See the text and footnote 46 for further discussion. The LIR strategist may also retreat to a second-order claim: CL (or whatever ideal logic we choose) regulates second-order disputes about validity. I doubt that would deflect the argument in the text.

<sup>45</sup> It is worth mentioning a distinction between two conceptions of the requirements of rationality made by Scanlon (1998: 30–31), namely, the one between being irrational (i.e. going against one's *basic* reasons, as it were) and failing to fulfil the requirements of ideal rationality (what one has *most* reason to do). For Scanlon, decision theory and deductive logic only set standards for the second notion of rational requirements. The worry I'm endorsing in the text is that it is questionable whether CL sets even merely ideal standards of reasoning.

<sup>46</sup> An argument of this kind regarding epistemic virtues is for instance in Williamson (2000: §8.7) and (2005), while his (2007: 92) is mellow. See also Christensen (2007) for further discussion.

But again, to think that this weakens the status of CL as the logic of reasoning is to confuse competence with performance (i.e. irrelevant truths are still truths and irrelevant truth-preserving transitions are still truth-preserving).

Now, I hear the appeal of a defence of this kind and I can see ways of adapting e.g. Zinda's (1996) notion of a linking structure between imperfect and perfect modes of reasoning to show how ideal standards of logical thinking can still have normative force for practices that routinely incorporate incoherent beliefs—the trick (and it's not an easy one to pull) is to make (formal) sense of the notion of *better approximation* towards the ideal.

Two obstacles however remain. Firstly, there is a difficulty here which is both highly specific and highly general, namely the very possibility of making sense of a notion of content determinate enough for us to make assessments about logicity (consequence is a relation between pieces of content: no determinate, truth-evaluable content, no relation);<sup>47</sup> if CL misses out on some subtleties of our thinking because it carves content too grossly (e.g. it's insensitive to relevance), that in itself is reason enough to ditch it (regardless of whether *any* replacement might fare better against the indeterminacy issue). And what needs showing in any case is that the discrepancies between competence and performance are genuine cases of *purely* cognitive imperfections and not, rather, *structural* failures of the modelling tools.<sup>48</sup>

Secondly, the possibility of fleshing out the notion of better approximation requires *antecedent agreement* on what would constitute ideal reasoning. This latter difficulty, that is, concerns the fact that it seems implausible that we can individuate ideal reasoning patterns that we strive to 'better approximate' without first coming to a decision *from our viewpoint as imperfect agents* as to what *would* count as ideal (for even in the philosophical closet we are far from infallible *when we draw up the regulative ideals of rationality*).<sup>49</sup>

In short, evaluating judgements about the status of the primacy of

<sup>47</sup> See e.g. the discussion in Williamson (1994: ch. 2).

<sup>48</sup> LIR, that is, should not disregard Chomsky's (1965: 9) metaphor that competence is the basis for performance. And yet it seems as if LIR all too often amounts to the claim that logic is *the* language of *pure* competence (competence *irrespective* of performance). That, however, would leave performance *ungrounded*.

<sup>49</sup> This again requires care: of course we have some kind of antecedent grasp of patterns of reasoning: that's *why* we find, when we do, certain aspects of CL implausibly idealised. The point I'm making in this paragraph is simply that *the very notion of approximation* is normative (it's easy to set up the maths to deal with *some* version of approximation; the hard part is saying why a certain function would be acceptable and another wouldn't).

CL (or of *any* other logic that would take its place on the throne) is not a matter that can be settled independently of judgements about what would count as ideal rationality—just reflect: what does the claim that ideally rational agents would obey the laws of the probability calculus amount to? *Who* set up that calculus? Was its precise shape given to us by some unforgiving god or was it the result of reflection on a reasoning base that can only be *both* the judge and the judged on such matters?<sup>50</sup>

I do not think that there are clear answers to these questions such that they could settle once and for all the question of *i*) which laws are genuinely logical (in the LSR sense) and *ii*) which are mere artefacts of a formal framework that, unless we can *antecedently and independently* establish the truth of a certain view of physics, *may* not have genuinely *general* normative import for us (for instance, outside of the mathematical domain).<sup>51</sup>

Nevertheless, if we endorse LIR and accept something like the better approximation principle, then it seems as if *the closer the gap* between the ideal and our approximating efforts the more likely it will be that the ideal *can* indeed plausibly be seen as regulating behaviour.<sup>52</sup> And so a logic that is demonstrably closer to our practices but that can still keep in place sufficient distance between our actual decisions and those that it mandates would be *ceteris paribus* preferable.

Whether *this* provides enough grounds to uphold (or dislodge) CL from its privileged position is an argument for another day.

Still, our ability to carry out meta-logical assessments over these

<sup>50</sup>I find the discussion in Sainsbury (2002b) highly instructive in this respect.

<sup>51</sup>That is to say: unless we can establish that nature is appropriately uniform, comfortably systematic and fully deterministic, we can always question whether the *laws* we posit for our various domains of discourse do in fact provide best fit with the facts (classic case: the old Quine-Putnam dispute over the import of quantum logic). Again, Lewis' (1982: 101) frankness is refreshing here: faced with genuinely radical challenges to the logical *status quo* (such as Routley's and Priest's), the classicist can only stamp her foot and retrench into dogmatism. But why should inconceivability (to David Lewis in 1982) be a guide to (timeless) illogicality?

<sup>52</sup>The LIR-arguments given in Williamson (2005: 480-1) strike an odd balance. On the one hand, we are told that it would be a mistake to expect an ideal theory to have practical import. On the other, we are told that such a theory would still enjoy normative force. In support of this, Williamson appeals to his usual anti-luminosity arguments. If transparency is not a determinant of best theory, however, it is unclear how we can even *choose* between competing theories. On what grounds is a theory that lacks *demonstrable* connections with our practices to be held to be *the* theory we ought to respect in our epistemic activities? As a parenthetical remark, I should perhaps add that I find it curious that the very same externalists who accuse internalists of over-intellectualising our *grasp* of concepts and more generally of the normative features of our practices should then commend to our attention regulative ideals that severely over-intellectualise those very *practices* (and not just our grasp of their normative features).

matters does depend on the tenability of some form of CET. And that's why an answer to the CET puzzle I discussed in chapter 3 is due.

The LIR strategy, then, is at the service of the choice of *some* logic as the regulative ideal. But any such choice must be justified on the basis of an argument that *it*, and no other, is the logic of choice for *us*. And I am questioning that this can be done independently of existing *theoretical* bias *and* of a solution to the CET puzzle.

Let me now conclude the discussion of idealising strategies by making two more remarks.

Firstly, it seems clear that our theory (our logic as science of reasoning) should have as much structure as the subject matter can support *and no more*, but also as much as it demands, *and no less*.<sup>53</sup>

If so, the generality requirement will force pretty severe restrictions on certain structural constraints that cause havoc when applied to natural reasoning (say, transitivity, reflexivity, exchange, dilution, idempotency).<sup>54</sup> For on any plausible reading of those constraints, they are in fact best construed as *local*, i.e. domain-specific constraints on reasoning (good in some domains, undesirable in others) and should not burden our inferential practices more widely.<sup>55</sup>

The upshot is that the idea of a truly universal logic, a logic that sets reasoning canons of absolutely unrestricted generality,<sup>56</sup> is unlikely to be achievable (by invoking LIR we lose applicability; by keeping applicability, we lose generality). The reason is that there are very few genuinely universal patterns of reasoning that can truly be said to *disregard* content (most likely none at all)—the resulting logic would therefore be extremely weak, and certainly much weaker than CL.<sup>57</sup>

<sup>53</sup>This is a principle that goes back at least to *Nichomachean Ethics*, I, 7, 1098<sup>a</sup> 25-29 ("we must [...] not look for precision in all things alike, but in each class of things such precision as accords with the subject matter, and so much as is appropriate to the inquiry"), and resurfaces in Peacocke's (1999) Integration Challenge.

<sup>54</sup>If we accept that importantly *logical* features of our everyday reasoning are best modelled by some form of dynamic, non-monotonic or resource conscious logic, then some (or all) of these structural rules will fail. See e.g. van Benthem (1996: 26), Marek and Truszczyński (1993) and Troelstra (1992) for details.

<sup>55</sup>Note that once again efforts to keep these structural constraints in place would involve a revision of the notion of content at hand. To deflect e.g. Girard's (1995: 2) argument against idempotency for 'and', that is, one would need to posit a richer notion of content (implemented by some variety of LFI- or of Clause-indexicalism) to keep track of the events involved (and thus discriminate between the two alleged co-occurrences of the same token).

<sup>56</sup>For one extreme form of this claim, one that includes illogical, inconsistent and paradoxical situations, see Routley (1980).

<sup>57</sup>As I discussed in the previous chapter, we *could* rig up domain-specific connectives that behave content-insensitively *within their domain* (a point pressed on me by Ole Hjortland and Colin Caret in discussion). The trouble for that suggestion is that, the case of quantifiers notwithstanding,



And even if we had settled on an appropriately weak and appropriately general logic, one crucial point would still remain: we pack into the meaning of the logical connectives *just as much* as we think serves the purpose of identifying what we take to be genuinely logical inference; but the issue of how much structure we *should* pack into those connectives is *not* independent of what we *decide* to treat as logical.

In short, and unsurprisingly, logic cannot tell us what logic *is* and which logic is the true one—there is no hope of detaching evaluative considerations from logical ones when we are testing both the tenability of LSR and its implementation through the selection of a particular (and privileged) set of logical tools.<sup>58</sup>

Secondly, I want to note that behind LIR there often (or perhaps invariably) lies the assumption that no other account (and certainly no weaker account than the classical one) would give us the required *systematicity*.<sup>59</sup>

We can now see the connection between the insistence on PoC and the LIR/LSR approach to our conception of logic. For it seems to me that the PoC rules do encapsulate both the idea that we can (and must) give a systematic account of *linguistic* practices (and *hence* of rationality as a whole) and that it is *those* rules (and nothing less than those rules) that can (and do) embody the logic of choice that regulates our thinking and our agenthood (here, finally, we come to the standard reply to the CLB question that I left hanging from the last chapter).

Imposing the specific PoC rules that one does impose on NL semantics is thus of a piece with imposing a logic on our thinking and our reasoning practices in general. And it is almost uncanny how the LIR strategy is invoked in precisely the same terms to defend PoC and UaGS on the one hand, as it is in the case of LSR and CL on the other.

But again, *whether* systematicity is to be imposed on a theory of logicality (whether there can be no reasoning and linguistic competence without systematicity) is precisely the point at issue and one on which logic itself cannot offer us counsel.<sup>60</sup>

logical connectives are supposed to be *domain*-insensitive too (their logicality is not supposed to be domain-relative, that is).

<sup>58</sup>See Field (1977) and Field (2000).

<sup>59</sup>See for instance the instructive conclusion of Williamson (2005: 483). I'm certainly overlooking all the subtleties in his account, but that conclusion, and the preceding discussion, seems to come close to saying: we give a description that doesn't fit our practices (or even explain them) but we do it because *otherwise* we would have no systematic account for the facts. Well, perhaps we should instead come to accept that there is no systematic account that would do full justice to *those* facts.

<sup>60</sup>I have to say that I find *pragmatic* justifications of the logical *status quo* particularly puzzling. In

It is thus no argument in favour of CL to insist that no other account would ensure systematicity. It is simply begging the question against the deviant logician (or against the theorist impressed by accounts that make no appeal to structure-directed systematicity) to insist that *precisely the level of systematicity generated by CL* is what is needed (let alone, that systematicity is *at all* needed to account for our practices in full generality).<sup>61</sup>

The *impasse*, then, is between two equally unfriendly options for the LSR defender: keep generality by weakening the logic and making consequence a notion that incorporates pragmatics into the very fabric of logic (recall that e.g. for the relevantist logician intensional, and thus non-truth functional, connectives are nevertheless *logical*: they *embody* relevance-sensitivity by imposing further constraints on classical consequence) or keep the logic classical and *defer* to pragmatics to adjudicate *when* a piece of reasoning apparently sanctioned by the logic can *in fact* be taken to be what *rationality* commands in a particular context.<sup>62</sup>

Either way, logic is made subservient to pragmatic judgement, thus contradicting the LSR dictum (there is something, namely, pragmatic thinking, *not* wholly regulated by logical thinking and in fact regulating it).

Accordingly, and contrary to what the traditional picture (and etymology too) seemed to assume, logic cannot be the seat of rationality, for its output is subject to pragmatic filtering from an authority (pre-

general, I'm far more sympathetic to constitutive accounts of our practices that spread rational constraints across the board, to include both our theories *and* their meta-theory (broadly construed). In fact, that's what I'm trying to do myself in the next chapter.

<sup>61</sup> Another quick example: take the dispute between the classical logician and her relevantist and intuitionist rivals. The relevantist will insist that the classical account of conditional reasoning is faulty. The defender of CL will reply that going relevantist would make mathematics and science a lot harder to discipline (it's much easier to let pragmatics explain away the paradoxes). But then in comes the intuitionist logician claiming that classical mathematics is incoherent (the classical meaning stipulations cannot determine a coherent practice). Were we to adopt intuitionist logic as the logic of reasoning, however, we would find that again it is very hard to give a proper model for natural reasoning. For we certainly do not use e.g. intuitionist disjunction in everyday reasoning, despite Ranta's (1994) and Dummett's (2004: 59) valiant efforts to make a case that we do. Logic then becomes a kind of short blanket being pulled in different directions (the logic that best models natural reasoning seems ill-suited to model arithmetic reasoning and viceversa). The point once again is that the defender of CL (and LIR) is imposing on everyday reasoning largely unnatural forms of reasoning that are justified only on the strength of the (very special) needs of mathematical reasoning (and *classical* reasoning at that). It may then begin to look as if the question of universality is ill-formed.

<sup>62</sup> As is familiar, defenders of CL will often explain away awkward cases by appeal to questions of assertibility and pragmatic felicitousness.

sumably) *above* it.

### 5.5. Situated Representation

I hope I've said enough to motivate a rejection of radical versions of LIR that insist on a model of competence that makes invalid reasoning acceptable—we cannot really burden pragmatics with the task of accounting for *all* discrepancies between idealised canons of reasoning and actual practice: some of those canons (e.g. monotonicity and paradoxes of implication) may well be not idealisations but misrepresentations of actual practice.

Alternatively, the orthodox logician will do a *modus tollens*: if the anti-CET and anti-LIR worries are sustained, then there can be no logic; but we clearly do have a logic on our hands, hence the CET worries are misguided.<sup>63</sup>

My view, however, is that we should instead do a *modus ponens* step here (you see, I do admit that there are logical constraints on our reasoning) and reflect on what the CET worries seem to teach us with regard to content and rationality, and of course, the shape of our logic.

I'll deal with the question of rationality in the next chapter. Let me now dig a little deeper into questions of content and consequence as I move towards my conclusion in this chapter.

To give you a sense of where I am heading, here's a slogan for you to mull over. Colin McGinn has nicely summed up one way to read Kaplan's account of indexicals (such as 'I'). The determination of reference in a particular context by means of a tokening of an indexical belief (say, one determined by a particular use of 'I') can be explained in terms of a *representation in context* rather than representation of the context.<sup>64</sup>

<sup>63</sup>I am aware of a tension in my argument here, for on the one hand I *am* adjudicating between logics while on the other I am saying that without a CET-guarantee logic could not even get off the ground. The point I am making is that, *assuming the truth of semanticism*, logic cannot be fully general, for too-finely cut content will make the CR empty (no genuine universal patterns of validity will emerge). But *as things stand*, CL's supremacist tendencies are in any case unjustified. I think these two claims of mine are fully compatible.

<sup>64</sup>McGinn (1982: 113). To recall remarks I made in fn. 71, p. 85, one may object to my construal of the (supposed) CET/LIR problem for semanticism by saying that I am in the grip of what Stalnaker (1984: ch. 2) called the *linguistic picture*, the idea that entertained content *must* have propositional structure as fine-grained as the means used to characterise its possession. Stalnaker (2008: 109–11) still pursues roughly the same line. To repeat: I dispute Stalnaker's contrasting pragmatic picture anyway, but moreover the problems I am raising here also apply to the descriptive efforts by the *theorist*, and not merely to the individuation of entertained content by the practitioners them-

I think this is exactly right, and I will be arguing that *linguistic representation itself* is also representation *in* a context and not *of* a context.

Where the CET theorist goes wrong, that is, is in trying to get semantic content (and semantic theories) to do two things at once: represent facts *from* a context-bound perspective while also including *all* facts *about* the context in the very means of representation of the perspectival facts so as to nail down content once and for all.

The point is: there is (and can be) no such thing as representation *of* a context, neither as part of semantic content, nor of the clauses given in the semantics. The representational powers of language *and* thought, that is, are far more limited than the traditional picture assumes (neither content nor theories *can* absorb context within themselves).

As a corollary, it is also the case that the idea of logical consequence as a question of *following from* no-matter-in-what-context is also incoherent (again, as standardly conceived).

Semantics, then, ought not to attempt to exhaustively capture context either into semantic content itself (the various CET strategies of context-semanticisation) *or* in the worldly representation that goes into the semantic clauses (the place where *all* the relevant contextual factors needed to *individuate* content get lined up).<sup>65</sup>

Clearly, this move is forced on the semanticist by the very claim that she is engaged in giving a truth-*conditional* semantics in the first place—the semanticist slogan after all is precisely that meaning is given by stating truth-conditions and that grasp of meaning is grasp of such conditions; and the natural way to construe truth-conditions is: *anything* that would make a difference to the truth of the sentence gets chunked in the clauses on *either* side of meaning-giving bi-conditionals. The move should nevertheless be resisted.

selves. Equally, they affect propositional structure itself, and not just mental content. Indeed, as I'll say in the next chapter, I want to give a *unified* account of (external) content ascription and (internal) content characterisation. See fn. 2, p. 163.

<sup>65</sup> Am I misreading the purposes of the Clause-indexicalist semanticists here? After all, it is one thing to claim that unarticulated aspects of content are controlled by variables at LF level and quite another to state the truth-conditions for sentences by including worldly elements in the indices. On the second strategy one is merely saying that a sentence is true *at* a world, time, location and so on. One is not claiming that the sentence is true *of* the world, time, location and so on. Maybe. But it seems to me that the essence of the semanticist project is precisely that by giving truth-conditions we state worldly conditions that are in some way or other *represented* semantically—when I say 'I am cold' I'm *saying* that Walter is cold *now* and in *this* world (not that the speaker is cold at *t* in world *w*). The line I'm pursuing here is also presented in Searle (1980, 1983a, 1994) and in Dancy (2004: §11.2). The idea that the content of a sentence is composed out of both the thought expressed by the sentence and the worldly conditions attached to it has recently been made explicit (and defended) in Wedgwood (2007: 59).

If the worries about CET I just rehearsed are on target, then, and in the face of the seemingly evident fact that we *do* succeed, by and large, in exchanging content and have made undeniable progress in getting to grips with the world, it seems to me that if we want to properly explain the role of logic in our practices what we require is a *revision of the notion of content* (and of its representational powers) that is (more or less explicitly) being used (or assumed) in the literature.

What we must give up in the first instance, then, is the MDP, the claim that *through meaning (alone) we can determinedly reach out to the world*.

### 5.5.1. On What Is Internal and External to Language

Now, if CET is false, how can we identify content? Already Aristotle was clear that questions of sameness and difference are essential to the identification of objects (and of our thoughts about them).<sup>66</sup> And so the question is: if CET fails, can we make sense of *partial* conditions on identity of content? Or should we rather go for a *criterial* view for such conditions?<sup>67</sup>

I will end up endorsing a weakly criterial conception of content in the next chapter. For now, let's stick with the attempt to give precise (if possibly partial) conditions on content individuation.

If we again turn to Frege we find that the justification strategy for his *Begriffsschrift* started by arguing that in order to think we need to use “sensible signs” and that the great discovery of the sign was precisely that it made it possible to call to our mind “that which is absent” (by which he primarily meant non-sensible thoughts and relations holding between them).

We thus seems to have a lethal cocktail of theses underlying the semanticist (and the LSR) project: namely, that logic (and the theory of meaning) are about giving an account of reasoning (and meaning) as a rational activity. And so we *must* make sense of one's reasoning (and one's use of language) *from the perspective of the user*, whence the requirement that a cognitive element be imposed on meaning, namely, sense.

Under semanticism, it's the sense of the connectives that directs our logical thinking; it's the sense of an expression that guides our ways with it; it's the (more or less radical) perspectival character of sense that justifies attributions of rationality in Frege's Puzzle cases.<sup>68</sup>

<sup>66</sup>See *Topics*, I, 5, 8-15.

<sup>67</sup>For the joys and sorrows of criteria, see Wright (1978, 1982).

<sup>68</sup>Again, McDowell (2005: 168, 185) is my guide here. Note that the problems I raise for seman-

Sense, then, is the route not just to reference, but to rationality as well (it is also its *root*: we are rational *because* of our ability to navigate the routes of sense).

But then, since the need to make us rational goes hand in hand with the need to make content precise, CET and (LIR) will *have* to kick in.

In thinking about the world, that is, we entertain linguistic items that purport to represent worldly items determinately. Take away CET, and it then becomes difficult *rationally* to justify our judgements as to the truth-value of our statements.

It is the semanticist after all who *does* routinely ask of her pragmatist opponent: *why* are some judgements more rational than others?<sup>69</sup> And one reply seems mandated: because they better track the *facts* and they can only do so to the extent that the sentences used at a context make determinate claims about those facts, or else, how could we assess them for fit?

It is at this point that the essential egocentricity of our thought—the fact that, in Perry’s (1986: 181) terminology, many aspects of our thought (and of the meaning of our expressions) are not *about* what we represent to ourselves but rather *concern* the object of our various attitudes—will *clash* with the need for cognitive guidance by the content of expressions.

Why? Because LIR itself has got to confront the issue that Frege had used to justify the *adoption* of a formal language to clarify aspects of content: we *have* to use signs to engage in thinking of any sophistication. But NL is not univocal and to that extent it exposes us to error.

Moreover, the logical laws are not written into language in such a way that compliance with *grammar* would guarantee correctness of thought process (which is indeed one way of reading the NS problem we discussed in chapter 2).

ticism are not lessened by going for a direct-referential account. I am not assuming here that sense *has to be* reference-determining descriptive conceptual content. If you are impressed by the sort of “lessons” that Soames (2005b: 329) thinks we should draw from the direct-reference revolution, then just think of sense as the competence-explaining *gelling agent* (for want of a better term) that keeps together (and *explains*) the essential social components of meaning and meaning-practices so beloved of that revolution (and of course there will *not* be *exactly one* such gluing agent). There must in any case be *something* that we are gesturing at when we hand-wave at those components; it may well be a purely *de re* piece of cognitive content (and incidentally, there is nothing in *this* story that is incompatible with Frege’s original view). The point remains that attributions of (linguistic) *rationality*, challenges to improper use of expressions, and illuminating accounts of the *distinctive* ways in which we are concept-mongering creatures in our use of words as in (just about) anything else, all require that *some* notion of sense be in place. And at *that* point, the CET worries will apply.

<sup>69</sup>See again fn. 193, p. 61.

Logical laws, Frege suggested, are laid externally to language, like a plumb line, and the whole purpose of formal languages is to take them in from the cold, as it were<sup>70</sup>—or at least to get the language-building to approximate the straightness of the plumb line as closely as the terrain will allow.

So, the main motivation for developing a *concept-script* was precisely the failure of NL to give us the tools to distinguish bad from good argument *purely on the basis of linguistic form*.

A formal language had therefore to achieve two purposes in one: incorporate logical laws right into its fabric and banish semantic underspecificity *by virtue of that incorporation*.

The point, however, is that as we saw in the previous two chapters CET *can't* be made to work, and *because of that* not only do we not solve the underspecificity problem, but we also do not solve the question of logicity.<sup>71</sup>

The move from NL to its formal counterpart(s), that is, does *not* remove the essential instability of the sign. The stipulations that supposedly fixed sense unambiguously are *always* formulated in a language that *cannot* be formal (the hierarchy of languages *always* bottoms out into the vernacular). And in any case a formal language is no less of a *language* for that.

In short, signs just won't keep still, no matter how much machinery you deploy to steady their roots. And crucially: too much machinery will in any case divest them of those egocentric traits that alone can provide cognitive guidance in our efforts to remain rational (this, really, is *the* key stumbling block on the way to a proper mapping out of the routes of sense).

In fact, I think that Frege himself finally came to realise this,<sup>72</sup> and my suggestion is that this is the way in which we should read his startling remark (which many have found terminally puzzling) that the essence of logic lies in its attempt to incorporate the *act* of assertion into language, failing to do so and *in the very enactment of that failure* managing, nonetheless, to *express* the essence of logic (the word 'true' fails to express the essence of logic, but its *failure* to do so succeeds in expressing that essence).<sup>73</sup>

Generalising Frege's insight, we could say that there are certain pro-

<sup>70</sup>The last image is mine; for the plumb line metaphor, see Frege (1882: 72/52).

<sup>71</sup>I.e. the determination of the non-logical/logical divide and of the extension of the CR.

<sup>72</sup>In his post-1902 "thunderstruck" state, Frege explicitly (and repeatedly) blamed the Basic Law V disaster on the misleading surface structure of NL. See his (1980: 132), (1924: 263) and (1924/25: 270).

<sup>73</sup>Frege (1915: 252).

cesses *essential to the communication of a precise thought* which language is not equipped to express, model or otherwise capture. These processes however are expressed (and indeed *represented*) *in the very act of attempting to express them*.<sup>74</sup>

Chief among these, the very *practice* of assertion: language cannot be isolated, abstracted and kept alive in a Petri dish, as it were—a *purely formal* language would not be a language at all, its signs would remain inert, they would indeed cease to *be* signs.

The contents expressed by that language would provide no rational guidance to us: *they* would be de-situated, but we are *essentially situated* (we engage with a world where there is no context-free truth) and furthermore our situatedness is something that eludes theoretical characterisation and indeed *linguistic* expressibility.

That NL is immersed in a practice that *alone* gives the signs life is a fact that is normally *hidden* in our everyday use of them.<sup>75</sup> Logic takes the signs out of that practice and makes perspicuous the *impossibility* that they could stand on their own feet.

To have made *that* evident is logic's greatest achievement, for logic's failed attempt to express the inexpressible does finally pin down the cognitive predicament we face in our quest for a rational grounding to our practices (or so I like to read Frege's gnomic remarks).<sup>76</sup>

It should be clear, then, that the problem goes beyond the issue of CET; or rather, the assertion problem is one more aspect of the CET problem.

Try as we may, we cannot encase *within* language not just content itself but also whether a particular sentence is being asserted seriously. For we might always raise the issue of whether assertion (the assertion sign) is some kind of stage direction for an actor rather than a *self-verifying* pointer that a sincere, earnest act of assertion is being performed. And assertion is what establishes the connection between content and truth; it is what transforms purely formal, toy-like languages into workable means for the genuine expression of content.

And here, finally, is where I think we should locate the cardinal mistake, the original sin in the very notion of *Sinn*, namely, the idea, a delusive one, that by switching to a *formal* language we can somehow

<sup>74</sup>In saying this, I think I'm still on the right side of the distinction that e.g. Salmon (1991) and Cappelen (2008: 265) draw between semantic and communicated content.

<sup>75</sup>On reflection, it is an astonishing (if not shameful) thing that the remarks along these lines in the *Investigations* (§23, 432) should have been seen as *revelatory* at all.

<sup>76</sup>It seems to me this is one of the points of greatest convergence between Frege and Wittgenstein. Compare *Tractatus* §4.121: "That which expresses *itself* through language, we cannot express by means of it".



overcome what are *constitutive* difficulties of *any* system of signs.

There is no language, ordinary or extra-ordinary, that can fix sense or extension with unerring specificity. Not even a language that only gods could use (LIR barks up the wrong tree in that respect). And this is because there are limits to the expressive powers of *any* form of representation (not even a god's eye view would be exempt).<sup>77</sup>

To put it another way: the limit is ontological, since reality *resists* univocal description (the very idea of “the view from nowhere” is incoherent, and indeed *sinnlos*, for it puts the notion of description beyond the bounds of sense, both perceptual and linguistic).

In this respect, language does its job *perfectly*: it reflects ontological complexities (and indeed obstacles to expressibility) in its very structure.<sup>78</sup>

Accordingly, LIR is mistaken because it assumes that there *could* be an idealised language (and a logic) that could escape those limitative results and that could thus *entirely* lack underspecification ‘features’.

And CET poses a final dilemma for the semanticist as she tries to secure LSR:

**SD VIII:** If CET can be implemented, then the LSR-generality claim entails that the CR is empty (finely-cut content will invalidate the generality of Tarski's structural constraints on that relation).<sup>79</sup> If CET cannot be implemented, then the LSR-formality claim fails, since we have no warrant for drawing the logical/non-logical distinction in the absence of the proper determination of content for the relata of the CR.

LSR, LIR and MDP-semanticism are thus incompatible theses to hold. Logic is, at most, the science of *minimal* reasoning over minimal content. To explain why (and how) we reason rationally under embedment we require *further* tools (and a different story—which I sketch in the next chapter).

More generally, Frege's (and the semanticist's) *grande illusion* was not just that there could be a *language* immune to underspecificity but also that *the thoughts we think* could be fully complete in the required sense—underspecification too, and not just indeterminacy, does be-

<sup>77</sup>Indeed, *this* is a way in which we could take the familiar and celebrated limitative results that characterise early-to-mid twentieth-century logic. I'm leaving it however open that those results may be related to properties of the *objects* of study, rather than the means.

<sup>78</sup>See again fn. 167, p. 52, and references therein for reasons to distrust epistemicist approaches to this issue.

<sup>79</sup>As we have seen, if e.g. vague content invalidates transitivity, our connectives will become domain-relative.

gin at home.<sup>80</sup>

Accordingly, if we want to give a proper account of rationality that takes the full measure of the CET-failure, we have to conclude that the objects of our attitudes are *radically and ineliminably minimal* pieces of content: call that *Radical Content Minimalism* (RCM).

The task for the next chapter is to make that notion as precise as I can. Before that, let me briefly sum up my discussion in this chapter.

## 5.6. Conclusion

Once again, my enterprise has been almost entirely negative. I have argued that the semanticist assumption that meaning determines truth-conditions, and hence content, is incompatible with the claim that logic is the science of reasoning. Sentential meaning is not determinate in the strong sense assumed by the semanticist—it does not suffice to lay out the conditions for its own truth. If so, there are no candidate pieces of content for the consequence relation to operate upon.

Given the impossibility to secure determinacy of content and to draw a theory-independent demarcation between the non-logical and the logical vocabulary, I have argued that, *as long as we insist that logic is a calculus of truths*, then the consequence relation will be empty (very roughly: transitivity fails because of vagueness, reflexivity and monotonicity fail because of the dynamic character of our reasoning).<sup>81</sup>

I have discounted idealising strategies and argued that they cannot *rationaly* establish the primacy of any particular logic in respect of its (putative) normative hold on our practices. The regulative ideal they posit is either too remote from our concerns or not rationally warranted by them.<sup>82</sup>

I have ended yet again by commending a radically minimal notion of content. Remove the aspiration to determinacy and much of our conception of logic survives, more or less unscathed, I claim.<sup>83</sup>

<sup>80</sup>For the latter, see Quine (1968: 46).

<sup>81</sup>Again, it would be no help to modify the claim to the conditional one that logic is a calculus of *potential* truths (a calculus of *place-holders*, that is). Given the failure of CET, we have no rational warrant for a *particular* transition from schemata to their instances.

<sup>82</sup>Purely pragmatic explanations for a given choice of logic cannot transmit rational warrant to their adoption.

<sup>83</sup>This is not quite the whole story though. What I would want to argue, had I the space, is that logic is best construed as the *sciences* of reasoning. I say a bit more about this in the next chapter, especially fn. 170.

We thus save Fregean logic but at the cost of losing Fregean content (we *must* renounce the idea that thoughts are the senses of eternalised, fully specified pieces of content).

So, while one facet of my conclusion is perfectly standard (consequence is indeed highly schematic), the other side is a little less familiar, for my view is that not just semantic content, but the very content of our *thoughts* is better seen as highly schematic too.

Unlike standard forms of minimalism, then, I think that content is *irredeemably* schematic: there is no completion in the offing, either for us or for the unseen gods of semantics (and logic). Thoughts are not propositions, nor are they propositional functions-like objects awaiting saturation-in-context.

My CET-free vindication of LSR is thus fairly obviously unfriendly to one key aspect of the Fregean project (my slogan: *sense is ineradicably thin*). I do not think there was any other option, though.

It is now high time I do something positive for a change. In the next and final chapter I turn to spelling out my minimalist proposal in a little more detail.

## Chapter 6

# When Authority Gives Out

### 6.1. Introduction

Throughout this dissertation, I have had one overall concern, namely, how semantics and logic can secure their constitutive normative hold on our practices by imparting an appropriate cognitive profile to meaning and reasoning.

The routes of sense, that is, are those of rationality;<sup>1</sup> they mark out accountable (and *discernible*) paths for our actions, they establish the network of correctly implemented movements of thought, they define our ethical stance in the world, they direct our self-constitution as agents.<sup>2</sup>

I have argued that *both* semanticism and pragmaticism make a mystery of that hold, indeed of its very *possibility*.

On the semanticist picture, linguistic (and reasoning) competence is but a matter of following the commands of the compositional machinery. The rational engine of language,<sup>3</sup> on that picture, is rule-based and recursive. All that we need contribute is the (largely passive) *recognition* of the appropriate meaning function attached to each expression and the (active) tracking of its deliverances-in-context.

Recursively generated output, however, is by definition something

<sup>1</sup>My view of sense-determination (i.e. of *novel* meaning computation) is exactly Wittgenstein's (1933-1944: III-43, 173): "The proof is not a movement but a route". It is (settled) sense-recovery which is a movement (there, we merely *rehearse* previously established proof-patterns).

<sup>2</sup>There are points of contact between RCM and some aspects of Bilgrami's (1992) conception of *individualistic externalism*, in particular his admirable attempt to reconcile the agent-relative explanatory role of sense with its world-rooted aspects. I join him in opposing the narrow/wide content distinction, but I'm not taking theories of meaning to be agent-indexed. For me, a theory of meaning states the minimal content attached to expressions as generated by an internal grammar that expresses practice-relative (rather than speaker-relative) contents.

<sup>3</sup>Edwards (1990: 185).

that can be followed, in Tennant's (1978: 6) memorable phrase, by "a comprehensively briefed moron".

One (embarrassing) problem with this view is that, intuitively at least, we flatter ourselves that a proper account of our condition would require positing a language faculty that delivers output fit not for morons but, rather, for *insufficiently briefed reasoners*.<sup>4</sup>

By hook or crook we use words felicitously in novel *and* familiar contexts in ways that transcend pure mechanicalness: we *decide* and *determine* fresh meanings or assent to the licit endorsement of existing ones in an open-ended variety of contexts that seem to resist systematisation, and *these* are the facts to be explained.

What calls for theoretical explanation, that is, is our convergence towards *negotiated agreements in judgement* as meanings are bent to *accommodate* contextual embedment, rather than our presumed obedient compliance with the putative superlative facts delivered by a super-rigid machinery.<sup>5</sup> It is *there* that the roots of rationality (and of sense) are to be sought.

Moreover, it seems obscure how we could possibly divine *which particular meaning function* is operative at a context, since the world is generally short on comprehensive briefings (Fregean meaning functions are supposed to be transparent to the user, but the uniqueness assumption—the semanticist idea that each expression has one and only one meaning function attached to it—makes them utterly opaque).<sup>6</sup>

Lastly, semanticism imposes a picture on us whereby language is

<sup>4</sup>The contrast I'm drawing here is no hyperbole. Semanticism assigns the crucial competence-explaining role to a recursive engine that delivers algorithms which are "of a peculiarly deadening and redundant sort", to borrow from O'Neill's (2000: 53) useful discussion of rules and principles of practical reasoning. Put pressure on their MDP claim, however, and (generalist) semanticists will qualify their claim and replace 'regiment' and 'determine' with 'constrain' (see e.g. Whiting (2007a) and the references in fn. 163, p. 51). No rule, they concede, could ever make provisions for all cases. Rules, that is, require supplementation (but not supplantation) by judgement—O'Neill (2000: ch. 3) and Dancy (2004) give a flavour of the parallel debate in meta-ethics regarding these issues. As I have indicated already and as will become clear in this chapter, I'm convinced that the answer(s) we give in meta-ethics will be the same as the one(s) we give in meta-semantics (and I don't think we have good answers on *either* side yet). One difficulty we face is familiar: reflective judgements on meaning-inferences will generate a Carroll-style regress; another difficulty is insufficiently rehearsed: in her retreat, the semanticist insists that we can make sense of meaning *functions* delivering a *unique* verdict attained via indefinitely many routes. This, I maintain, is a fantasy. The difficulty for semanticism is not the multiplicity of the routes of sense: it's rather in the idea that, as required by MDP and UaKTC, those routes all lead to the *same* endpoint.

<sup>5</sup>The jargon here is intentionally Wittgensteinian but Chomsky's (1966: 62) view of CC is surprisingly very close in spirit.

<sup>6</sup>The point has been convincingly made in Travis (2006b: ch. 3).

claimed to possess the capacity to secure determinacy of content (the MDP again). That claim, however, flies against the evidence of the intrinsic *expressive limitations* of language and the ever-changing variety of the world we interact with (neither our words nor our world are as determinate as the semanticist presumes).

On the pragmaticist picture, by contrast, linguistic indeterminacy manages, by ways just as mysterious as the semanticist's, to reach out to perfectly determinate thought contents. Semantics is sub-propositional, but thought is fully propositional, we are told. The precise character of what bridges that gap is however left mostly unspecified. And as I argued in chapter 3, the little that *is* specified adds up to an incoherent framework.

On neither account do we come out as fully rational, then, and our competence with signs remains still a mystery.

My final task in this dissertation is to suggest a way out of the difficulty. My proposal is that we take *both* meanings and thoughts to be radically minimal entities. I'll fill in the details from section 3 onwards. First, though, I need to say something about the normativity of meaning, for the very idea that semantics may have normative import has come under pressure recently.

## 6.2. Normativity and Its Sources

Wilfrid Sellars once wrote that “[t]o say that man is a rational animal, is to say that man is a creature not of *habits*, but of *rules*”.<sup>7</sup> We are reasoning creatures, and reasoning is not (and *ought* not to be) the mere treading of settled ground. Rather, being rational is a matter of obeying rules that must be available to conscious reflection and recognisable by reasoners as the norms that regulate one's practice.<sup>8</sup> In short, what defines us *qua* seats of rationality is our peculiar sensitivity to the demands placed on us by rules that purport to take us beyond mere regularities in behaviour—there is no explaining what we *are* without appeal to rules.

For what rules do is partition the class of possible actions into rea-

<sup>7</sup>Sellars (1950: 138). Sellars' conclusion (p. 155) was that rules are largely ineffable (mixing, or so it seems, early and late Wittgenstein). Talk *about* rules, on this diagnosis, is nonsensical because rules are to be *lived* rather than *described*. For an attack on rule-based accounts of rationality, see e.g. Putnam (1981: 124-26) and Rorty (1985). Edwards (1990), in particular pp. 51-63, gives a thought-provoking take on the rise and fall of the view of human activity as rule-governed.

<sup>8</sup>Rules of reasoning, that is, are those that *guide* us, not merely those to which our behaviour does (perhaps wholly contingently) conform (a distinction first mooted in Quine 1972: 442).

sonable and unreasonable ones,<sup>9</sup> and by so doing they define a practice (in pretty much the same robust sense in which rules of inference, and/or of proof, define a logic by determining its class of validities) while also specifying what it is to be a participant in that practice.

Both tasks are achieved not just by setting clear-cut standards for rationality to which practitioners will be answerable, but also by laying down *what it is* to be a move in the reasoning game: change the rules and you change the game; change the logic, and you change, allegedly, the subject. There is, it seems, no *escaping* the domain of rules, if one is pledged to rational self-constitution: forsaking rules altogether is forsaking our essence (for there are no alternative *implementations* of rationality, nor of logic).

Accordingly, rule-governed behaviour is precisely what *exemplifies* a practice *as the practice it is*, and one's rationality will then be assessed against the background of the rules that are taken to be in force within that practice. We are rational, and interpretable as such, because (and to the extent that) we are sensitive to reasons as provided by rules;<sup>10</sup> rules-as-reasons, that is, generate standards-setting *oughts* in appropriately responsive beings.<sup>11</sup> Reject *all* rules, reject the claim that *they* hold normative force over *us*, and you place yourself outside humanity.<sup>12</sup>

The master thought here, whether or not expressed in Sellars' terms, is familiar from discussions in, *inter alia*, metaethics and appears irresistible—what *else* but submission to rules *could* make us the distinctively rational beings we are?

Rules, however, are (by definition, it seems) *given*, not found;

<sup>9</sup>Here I'm thinking of 'actions' as the purposeful taking of steps of whatever kind, to include purely theoretical reasoning.

<sup>10</sup>I borrow the notion of sensitivity to reasons from Raz (1999: 11). Since at least Sellars (1956), it is now of course a completely standard notion. See e.g. Scanlon (1998: ch. 1), Skorupski (1997, 2010) and Parfit (2011: ch. 1).

<sup>11</sup>Relatedly, we are sensitive to reasons to the extent that we are sensitive to value—that we find something valuable is a reason for action. See e.g. Raz (1999: 1), Korsgaard (1996: 7). Some form of self-constitution is to be found in most meta-ethical accounts, from neo-Kantian constructivists (Korsgaard 2008, 2009 and O'Neill 1996) to contractualists like Scanlon (1998). Even expressivists like Gibbard (1990) require that our lives be defined by one's endorsement of a system of norms before they can count as fully rational. Norms, in other words, are self-given and this is an important part of the distinctiveness of our situation in the physicalist's world. Moral realists such as Shafer-Landau (2003) or Wedgwood (2007) would of course object to my characterisation.

<sup>12</sup>And would it even be *possible* to reject rules altogether? The very idea presupposes *systematicity*—a globally negative attitude with respect to rules, that is, is itself a rule (of sorts). The point here is similar to the idea of the inescapability of agency: we cannot escape agenthood, and it is constitutive of agency that it be sensitive to norms of rationality. See Ferrero (2009) and Enoch (2011) for a recent discussion.

to take us beyond sheer habit, to be more than *mere* dispositions (whether brute or genteel) they require a (highly pronounced) degree of detachment from purely naturalistic properties.

And so the next pressing question usually raised is: whence the *source* of the normativity that seems required in order to ground the rules by which we define ourselves? Not just any old rule will do: we need rules grounded in the *right sort* of normative authority, rules that deliver the right account of rationality in the various domains.

Given the paucity of immediately available naturalistic reductions, an equally familiar answer has been: reflective scrutiny, whether or not tinged with Kantianism, will yield the most plausible (i.e.: more easily vindicated) candidate(s) as the source of the normative commands under which we labour.<sup>13</sup>

In other words, we more or less self-legislate ourselves into being by acknowledging rules that disclose their character only under reflection.<sup>14</sup> Fully-fledged rationality, that is, is not just a matter of purposely following rules; it also requires that we (be able to) examine the pedigree and grounding of those rules, perhaps tracing them back to the very nature of thinking itself. Indeed, we may even conclude not just that we *must* follow rules to be rational, but that we *are* the rules, or at least: that we are their privileged vehicles—the rules, that is, tell us deep facts about the very structure of our minds.<sup>15</sup>

### 6.2.1. Rules of Language

There is one area where the question of normative grounding is particularly urgent while also presenting itself as very nearly intractable, namely, language. At first blush, language seems to be the very

<sup>13</sup>See e.g. Onora O' Neill's introduction to Korsgaard (1996: xiv). There are of course non-Kantian variants of this thesis, or radical alternatives to the reflective view, such as the account of desire-based normativity sketched in Finlay (2007). My sympathies, however, lie unashamedly with broadly constructivist approaches.

<sup>14</sup>It may be objected that reflection is also needed to disclose fully naturalistic properties. To use Sellars' (1960) terminology: the move from the manifest image to its scientific counterpart is never immediate, that is. I grant that point. But what is distinctive about normativity is that reflection is *intrinsic* to its grounding—when (if) we finally uncover the bedrock of our norms, we do find our reflective efforts built right into its layers (even on a realist construal). Or at least: that's what strikes me as the most plausible story we can tell about that grounding.

<sup>15</sup>This seems to be a fairly reasonable gloss on e.g. Chomsky's insistence that the structure of linguistic representations (the internal setting of the local linguistic parameters required by the universal principles underpinning the language faculty) discloses biological facts about the architecture of our mind.



paradigm of a rule-following activity.<sup>16</sup> And yet, the attempt to establish a grounding for its authority over our practices soon lands us into deep paradox, as we shall presently see.

Now, if the picture above is accepted, it looks as if a necessary condition on rationality is the ability to follow rules (and nowhere more so than in language); but then, a convincing answer is owed to the intricate issues raised by Wittgenstein's rule-following considerations (RFC). For on one reading of those considerations it seems to be quite impossible to follow rules at all, or at least (but no less devastatingly): it seems impossible to do so in the manner assumed by the standard picture—what the RFC challenge, that is, is the very idea that rule-following be a *rational* activity.

The RFC paradox, ultimately, highlights the deep tension between the insistence that we are rational to the extent that we have the capacity to follow and respond to rules and the realisation that the *only* workable explanation of that capacity is a heavily naturalistic one: that's what we do, that's the way our brain is structured. We are thus caught between the need for a rule-based account and the fact that rules have no normative basis themselves (the *ought* bottoms out into the *is*).<sup>17</sup>

Hence, unless we can show where Wittgenstein's reasoning went astray, our cherished conception of rationality appears to be under terminal threat, since Wittgenstein's own conclusion was that rule-following *mandates* that there be a (more or less pervasive) layer of *blind* reasoning built into our practices—and reasoning conducted in utter darkness is not an activity, it seems, where rationality may flourish.

I'll return to this issue in section 6.4. For now let me try to secure the thesis that meaning-rules are appropriately normative.<sup>18</sup>

### 6.2.2. The Normativity of Meaning

At least since Kripke (1982) it has become commonplace to speak of meaning as being normative, thereby providing the old commonplace view that language is a rule-based activity with a fresh, more

<sup>16</sup>Without rules, we lack *the* means to distinguish noise from speech, or so argues Blackburn (1985: 213).

<sup>17</sup>It is of course an interesting question whether only *normative* facts (of the kind sought by the moral realist) can ground normative constraints on our practices.

<sup>18</sup>I need that in place, since my account insists that grasp of content occurs under normative, non-naturalistic constraints.

substantive slogan.<sup>19</sup>

More recently, however, the contention that meaning is a normative notion has come under increasing challenge.<sup>20</sup> Very briefly, the main problem is supposed to be as follows.

If we take the normativity thesis (NT) to involve the claim that meaning is *essentially* normative, then we should expect (at least) two things: 1) that from a meaning specification a *binding* prescription for use should straightforwardly follow (either immediately or via inference); 2) that breaches of the rule would *immediately* amount to changes of meaning (for a violation of *semantic* norms should directly affect the semantic content of an expression).

In other words, if the NT holds, only *correct* uses of an expression can *convey* (i.e. *individuate*) its meaning (and perhaps more strongly still: if it holds, the meaning of a word is the equivalence class of all and *only* its correct applications). According to the NT, then, *when-ever* you disregard the applicability conditions attached to the norms for use, you are thereby changing the meaning of the expression. Or so the objection goes.

What is supposed to make trouble for the NT thus stated is that fairly often we do use expressions in utter disregard of their correctness conditions, and this can happen for a wide variety of *non-semantic* reasons; *pragmatic* factors of various kinds, that is, may (and do) trump compliance with those conditions, or may even make it impermissible for *moral* reasons.<sup>21</sup>

The point of the objection is that *even in those circumstances* it would not follow that the term is not being used in full respect of its meaning. It is uncontentious that in those circumstances we misapplied the term, but misapplication, in and of itself, does not weaken the standing meaning stipulations. On the contrary. For there is no question that the breaches of the meaning-norms attached to an expression leave our *competence* fully intact.<sup>22</sup> And this, the objector

<sup>19</sup>Brandom (1994: 10) attributes to Sellars the idea that meaning is “fraught with ought”. Gibbard (1994: 96) has even grabbed the opportunity thus provided to metaethicists to claim philosophy of language as their (sub)-province. I commend the move.

<sup>20</sup>See e.g. Bilgrami (1992: 110-113), Glüer and Pagin (1999), Wikforss (2001), Glüer (2001), Boghossian (2003, 2005a), Horwich (2005: Ch.5, 6), Hattiangadi (2006, 2008), Glüer and Wikforss (2009). For a defence of the normativity thesis see Whiting (2007b).

<sup>21</sup>Misapplication of terms may indeed be *the right thing to do* when engaging in irony, deception, white lies and so forth. And in all such cases we disregard the norm while retaining mastery of the expression: or rather, the very act of disregarding the norm exploits the meaning-specifications themselves.

<sup>22</sup>Note that it is not just purely local, episodic breaches that do not impinge on the force of the standing meaning-norms. We can easily imagine a perverse speaker who, although fully conversant

concludes, shows that *at best* semantic normativity is hypothetical in character.<sup>23</sup> Our obligation to comply with semantic norms, that is, is entirely conditional on our other desires and interests.<sup>24</sup>

The objection, then, grants that correctness conditions do attach to expressions, but insists that from those conditions no categorical command can ensue unless additional, and substantive, extra-semantic premises are added. If meaning is normative, then, it is not *essentially* so. That is, *contra* the NT, meaning's sway over our practices is not *exhausted* by its alleged normative character (something more is needed for normative constraints to be attached to the use of an expression than the purely semantic stipulations associated with it).

To put it another way still: meaning clauses given in normative terms will not *individuate* (or define) meanings appropriately; they will not capture *all* occasions of use of an expression with *that* particular meaning attached to it, if determination of meaning is seen as *purely* normative in character (more precisely, if the normative constraints attached to the expression are limited to a specification of its correctness conditions).<sup>25</sup>

Now, that from correctness conditions, as normally stated, no categorical obligations ensue is undeniable, as far as it goes, but also relatively uninteresting. It seems fairly clear, that is, that a form of internalism about motivation would be extremely implausible as an account of linguistic competence, at least as far as the NT-proposal currently stands. If we e.g. judge that expression *e* means  $\mu(e)$  (for some meaning-function  $\mu$  mapping syntactic strings to a domain of meanings), it does not seem to follow, on any reasonable view, that we ought to be compelled to use *e* *only* in the circumstances mandated

with the meaning-stipulations for an expression, *systematically* and wilfully misapplies the term. The point is that even to make sense of the case we have to accept that the norm is fully salient to the speaker (and its salience is indeed required for the agent to be able to *direct* her obstinate deviancy). Linguistic *akrasia*, that is, is a perfectly imaginable scenario that unlike its intentional counterpart triggers no contradiction, at least under the proposal discussed in the text.

<sup>23</sup>See the references in fn. 20. Hattiangadi (2006: 220) has a useful bibliography for the two positions.

<sup>24</sup>There is a stronger conclusion often drawn, namely that semantic norms *qua* semantic norms either do not place *any* obligations on language speakers, or if they do, they do so in a supererogatory manner. Bilgrami (1992: 111-3) seems to be pushing for a conclusion of this kind, no doubt under the influence of Davidson (1986). This seems to go beyond what the anti-NT argument has so far established, though.

<sup>25</sup>For Davidson (2001: 297), all that meaningfulness requires is "a considerable degree of consistency in the use of words", and no more. And that, he adds, is a practical norm, not a semantic one, and it arises from a desire to be understood and not from intrinsically semantic obligations. Similar points are made in Bilgrami (1992: 112-13).

by the conditions specified by  $\mu(e)$ . A proper theory of linguistic action requires a much more sophisticated form of internalism than this crude proposal does allow,<sup>26</sup> and in any case the early proponents of the NT were already clear that meaning stipulations furnish only hypothetical reasons for (linguistic) action.<sup>27</sup>

The difficulty for the defender of NT is supposed to be that meaning-specifications are not categorical in the sense that they require integration by non-semantic desires of various kinds before they can *move* agents to (linguistic) action.

NT is thus seen as a form of *meaning rationalism*: semantic norms provide reasons not just to form beliefs about meanings but they also, and *eo ipso*, make available ought-reasons to the agent with respect to the lawful employment of expressions.<sup>28</sup>

Clearly, we should concede that non-compliance with semantic norms is *not* a case of outright *meaning*-deviance. The non-compliant speaker is not being irrational in her *meaning*-judgements (linguistic *akrasia* is *sui generis* weakness of the (linguistic) will),<sup>29</sup> whereas if the NT were true it would seem to follow that a charge of irrationality should be in order (if meaning-specifications carry purely semantic normativity, then failure to comply should offend against one's better semantic judgement and yet intuitively it does not do so).

Let me now offer a revised version of NT that should be able to address the issue of the supposed inertness of semantic judgement with respect to action. If sound, my proposal will end up making non-compliant speakers irrational after all.

### 6.2.3. Meaning as Expectation

The challenge we face is to explain how norms can individuate meanings and *thereby* also provide essentially semantic reasons for com-

<sup>26</sup>Discussion of internalism about reasons/motivation is lively in contemporary metaethics. Shafer-Landau (2003: ch. 5-7) is a useful starting point.

<sup>27</sup>As Miller (2006: 109) notes with respect to Kripke and Boghossian.

<sup>28</sup>'Meaning rationalism' is a label due to Millikan (1993b). In her use, this is intended to indicate a commitment to the generalised *scrutability* of sense to the agent. In contrast, my use here is intended to refer to rationalism about *reasons*. Meaning rationalism, as I use the term, is thus the view that grasp of meaning suffices to provide semantically-based ought-reasons to use an expression in a certain way. So, if the speaker judges that an expression *e* means *F*, then either she is motivated to use *e* to mean *F* or she is irrational (and on my view, she is not just practically irrational, for she is instead to be convicted of an inconsistency of pure rationality. In the next section, I explain how this can be the case).

<sup>29</sup>I'm assuming *akrasia* is to be equated with weakness of the will, which, at least since Holton (1999) has been called into question. And I'm also assuming that the proper analysis of *akrasia* is a charge of irrationality against the akratic subject, which again might be questioned.

pliance with those specifications. We want to secure, that is, *intrinsic* normative valence for expressions, a normativity that derives *entirely* from semantic considerations (and not from *pragmatic* ones)—the routes from sense to action must be entirely semantic in character.<sup>30</sup>

My suggestion is that we should think of semantic norms as coming in pairs.<sup>31</sup> For each expression in the language, that is, there are *norms of expectation*, which are inviolable on pain of meaninglessness (and indeed of irrationality), and *norms of fulfilment*, which can, and routinely are, infringed on pragmatic grounds.

What a semantic norm primarily prescribes is *what one should expect the word to be used for* (i.e. the set of circumstances where its sincere use would be warranted):<sup>32</sup>

**ExpNorm** Necessarily [for any  $\mathcal{L}$ -speaker  $S$ , for any  $\mathcal{L}$ -expression  $e$  ( $e$  means  $F$ -in- $\mathcal{L}$  only if (absent reasons to the contrary,  $S$  will expect (and be expected) to apply  $e$  only if conditions  $C_F$  are seen to be satisfied))]<sup>33</sup>

All other things being equal, the expectation will be fulfilled. Violation of the norms of fulfilment, however, is to be countenanced and indeed *rationally* anticipated (since fulfilment-defeaters abound in the most normal of cases, triggered by, as well as triggering the standard Gricean mechanisms).<sup>34</sup>

Norms of expectation, however, are *constitutively* inviolable by participants in the language game. To violate *those* norms would be,

<sup>30</sup>As Hattiangadi (2008: 55) makes clear, the objector intends to exclude a wide class of non-semantic considerations, including prudential, moral, legal ones, as well as communicative intentions. It is an interesting question whether one can espouse some form of intention-based *semantics* and yet placate an objector who takes Hattiangadi's line.

<sup>31</sup>I'm taking a cue here from remarks in Husserl's *Sixth Investigation* (1900-01/2001: ch. 1, §10), Heyting (1931: 58-9), Wittgenstein (1930: §33) and (1945/1953: §445). Smith (1994: 85-7) speaks of expectations with respect to moral norms.

<sup>32</sup>One might worry here about the beginning of a regress problem for a view of rules that makes their applicability conditional on the *antecedent* recognition of the obtaining of certain conditions. The regress problem is one that has been much discussed in the meaning normativity literature and all I can do at this stage is point towards the recent discussions in Boghossian (2001, 2005b, 2008b) and Wright (2001b). Sellars (1954) is also (still) relevant.

<sup>33</sup>I am not entirely sure how to fit speakers' judgements in the definition, since a charge of irrationality against those who flout **ExpNorm** will only stand in case  $S$  judges that  $e$  means  $F$ -in- $\mathcal{L}$  and that  $C_F$  obtain. Perhaps the stipulation that  $S$  is in relevantly ideal epistemic conditions would suffice.

<sup>34</sup>See Boghossian (2005a: 97) for the obvious point that we often lie, set out to deceive, joke or what have you. Other more outlandish cases of norm violation are also discussed in e.g. Hattiangadi (2006).

truly, to change not just the meaning of an expression (a conceptual impossibility, given the definition), but the entire language game too.<sup>35</sup>

Or rather: it would mean placing oneself outside humanity, and indeed consigning oneself to *idiocy*, in the original (Greek) sense of the word, since the putatively deviant *S* would simultaneously judge that *e* means *F* and that there is *no* expectation that she would use it to mean *F*. It seems, that is, a conceptual truth that to mean *F* by *e* is to expect that *e* be taken to mean *F* on an occasion of use (quite regardless of whether the applicability conditions are *actually* met).

It is expectation conditions that individuate content; it is expectation conditions that set the standards for linguistic competence; it is expectation conditions that make deviancy with respect to the standards *they* set truly irrational. To understand an expression is *ipso facto* to understand the expectation commitments incurred by its uses.

What recent discussions of meaning-normativity seem to have overlooked, then, is precisely the fact that meaning-norms are *composite*: the exclusive focus on applicability conditions has distorted the debate about these issues. Or so I have argued in this brief sketch.<sup>36</sup>

Accordingly, it is a conceptual truth that meaning is given in terms of expectation conditions, and those conditions are categorical—unlike correctness conditions, any violation of expectation conditions offends against *purely semantic* normativity.<sup>37</sup>

<sup>35</sup>For the game we play is one where it is essential that norms of expectation be respected, on pain of global incoherence for the practice—in Gibbard's (1990: 65) terms, you may think here of linguistic cooperation as *coordinate expectations*. To mean *F* by *e* is to (defeasibly) expect *e* to be used to mean *F*. Norms of expectation, then, allow us to recognise a token as a token of its type, in exactly the way in which we identify the King in chess as *that* particular *Spielfigur* and not the actual "bit of wood" on the board (*Investigations* §35). The distinction between norms of expectation and norms of fulfilment is similar (but not identical) to Searle's (1969: 33) distinction between *regulative* and *constitutive* rules (the former regulating antecedently existing behaviour, the latter bringing a practice into being). Norms of expectation are constitutive precisely because to disregard them is to annihilate meaning: by breaking them, you've indeed changed the subject; you're no longer using *signs* (Zettel §320).

<sup>36</sup>Another way of putting the point is that norms of expectation are norms about competence, whereas norms of fulfilment are norms about performance.

<sup>37</sup>We could imagine objections to this account along these lines: the account makes meaning mysterious, inaccessible, private. To which the proper reply is: certainly not, for the very possibility of communication requires a fixed point in our conversational scorekeeping. Expectation conditions are the rock on which everything else stands. That they are in place is a *detectable* condition, fully manifest in the linguistic behaviour of the participants. Of course, expectation conditions are just as vulnerable to RFC as any version of the language-as-rule-governed hypothesis. But that's a

#### 6.2.4. From Norms to Rules

If what I've said so far is on the right track, it seems to be both possible and necessary to give an account of language as an activity defined (and policed) by rules for use that do carry categorical normative force.<sup>38</sup> In the next section, my concern will be with what grounds their authority. In this section, I want to briefly revisit the much-discussed issue of the Wittgenstenian RFC.

As we saw in chapter 2, PoC commits the semanticist to the claim that the compositional rules and the lexicon will *mandate* a verdict by which we have to abide (meaning rationalism *is* the contractual view of meaning).<sup>39</sup>

Now, PoC in effect makes exactly the sort of claim that the RFC are supposed to have shown to be incoherent, namely, that we can *rationally* project from a finite basis to an open-ended totality of yet-to-unfold use. The RFC-lesson, allegedly, is that there could be *no* rules able to effect the transition from a semantics for the basis to one for the complex (and indeed viceversa)—or, equivalently, that if there *could* be rules of that sort, we could have no epistemic access to them. In other words, if the RFC are correct then the EP has no solution.

Moreover, **ExpNorm**, as I have stated it, is (partly) about intentions: attaching a certain meaning to *e* entails forming a certain expectation-intention. As such, it is immediately vulnerable to the RFC, which famously attack the ability of any norm to establish an *internal* relation between intention and future use.

But we needed **ExpNorm** in place to defend the NT, which in turn seems to be required to make our linguistic practice a matter governed by rules-as-norms and not by habit (something that I am committed to). And so the conundrum is that if we want to defend a view of language as a rational activity (one where our beliefs about meanings are accountable to clear standards of rationality) we need to appeal to something like **ExpNorm**, which in turn opens up the

different worry altogether.

<sup>38</sup> A note of warning: I am not siding with *prescriptivists* about grammar here. In fact, my conclusion will be quite the opposite of a prescriptivist view. All I am saying is that for language to occur there have to be *some* norms concerning meaning-expectations. It is consistent with that position to maintain that the privileged status of those norms is provisional. One might object that Exp-Norm does not really give norms of *outward* use and is thus no better off than traditional accounts (in fact, it completely by-passes the problem). Short reply: unlike use-dispositional accounts, Exp-Norm puts very precise constraints on an explanation of fulfilment *failure*.

<sup>39</sup> To adapt a vivid image from McDowell (1993: 274), the compositional machinery is *already* waiting for us at the end of the meaning-computation ready to pass judgement on our performance as we process complex meanings.

flank to the RFC.

The additional problem is that even if we managed to secure the internal connection between intention and future use, there is good evidence (or at least, evidence that suffices to give pause) that the kind of rules that could attach to expressions (including the connectives!) cannot make provisions for *all* cases.<sup>40</sup>

I'm here merely highlighting the difficulties for the thesis I'm defending. I'll try to provide an answer to the RFC in section 6.7. First, I need to discuss a further problem we face in this area.

### 6.3. The Paradox of Authority

Let me take stock briefly. We started off by noting that a proper account of what is distinctive about our practices seems to require norms and rules to be in place, rather than just habit-induced propensities. I tried to stabilise a view of linguistic norms that would allow language to qualify as a fully rational activity, governed by meaning-determining rules. I then raised the question of the impact of the RFC on all of this.

I now want to consider the question of the authority of language in a little more depth.

Language poses a special problem about the grounding of its norms.<sup>41</sup> If the sources of those norms are themselves norm-laden entities, then the origin of those sources cannot, or so it seems, refer back to either the norm themselves or the practitioners involved. A naturalistic reduction of those norms—the obvious non-Platonistic strategy, one would think—would however face the familiar problem of justifying a transition from a *biological* 'is' to a mental/linguistic 'ought',<sup>42</sup> and that's precisely why we needed the NT in place.

And so we have (at least) two paradoxes in this area, the second one generated by the attempt to escape the first (that is, the RFC one). The shape of the second paradox is well brought out by Dummett in his William James Lectures:

<sup>40</sup>That's Zettel §440 again.

<sup>41</sup>The problem, in fact, is perfectly analogous to that facing the foundations of ethics. That's why I agree with Gibbard's judgement that the theory of meaning falls largely under the scope of meta-ethical reflection (see also Russell 1940: 238).

<sup>42</sup>Dretske (1998: 245-46). One might of course be perfectly untroubled by this and suggest that there are such things as biological norms *à la* Millikan (2005). Or that there are Platonistic accounts that are non-reductive but epistemologically 'sensible', where reflection on facts about essences and conceptual structure provides the required grounding, as with Wedgwood's (2007) notion of normative dispositions.



The paradoxical character of language lies in the fact that while its practice must be subject to standards of correctness, there is no ultimate authority to impose those standards from without. The only ultimate determinant of what the standards of correctness are is the general practice of those recognised as primary speakers of the language.<sup>43</sup>

We are forced into this paradox (just as into the RFC one) by the observation that the requirement that norms be in place is a precondition on meaningfulness:

if there cease to be right and wrong uses of a word, the word loses its meaning.

So, it looks as if a) we need rule-based normativity to be in place for expressions to be meaningful; but b) rule-based accounts give rise to paradoxes about i) the grounding of the rules and ii) our ability to follow them.

The rule-requirement, then, appears to generate near-intractable paradoxes, and meaning skepticism (if not outright nihilism) seems the only likely outcome. But the problem about language is not just special: it is *urgent*, for without a solution to it, we lack a clear account of how we can even *think* under rational constraints, and if so it is unclear how *anything* we do can ever be considered rational.<sup>44</sup>

It thus seems as if we have no option but to confront the RFC issue head-on, since making sense of the possibility of rule-governed behaviour is arguably *the* task that we have to confront as self-reflective beings.

I need to discuss one more issue before I can move on to sketching my own position.

### 6.3.1. A Response-Dependence Account of Linguistic Judgements

At the end of the *Reflections on Chomsky* volume edited by Alexander George in 1989, Crispin Wright put forward the suggestion that

<sup>43</sup>Dummett (1991: 85). Here's another nice paradox: 'vernacular' comes from the Latin 'verna/vernaculus', the tag given by masters to slaves born in their home. There you have it: to the extent that we are born into it, we are both masters and slaves of our vernacular.

<sup>44</sup>One worry often raised in Boghossian (2008a) is precisely a regress of content determination: we'd need to have contentful items already in place as we try to ground content itself. No non-circular analysis seems possible. And there is a related worry in this area well-expressed by Gibbard (2003: 86): "What I am thinking is a matter of the rules I am following in my thinking." So, if an appeal to rules cannot be made to work, the very act of thinking would be paralysed by the absence of rules, for the very act of meaning individuation on which, presumably, the entertaining of content itself depends, is in turn dependent on the existence (and observance) of rules.

one might escape the RFC bind by conceiving of semantic content as *secondary* in character.<sup>45</sup>

The idea, very roughly, is that the properties that attach to NL strings are best conceived as incorporating our *responses* into their very fabric, by analogy with the familiar post-Lockean conception of colour predicates.<sup>46</sup>

In particular, Wright's proposal was that *when stating truth-conditions* for sentences we should "write human responses into the account"—hence the label *Response-Dependence* (RD).<sup>47</sup>

The objectivity of meaning (whatever *that* means) would then be defined in terms of *best opinion* (opinion reached by ideal participants in ideal conditions, i.e. in an unimprovable information state).<sup>48</sup>

Accordingly, our judgements about the semantic properties of sentences would be *extension-determining* rather than *extension-reflecting*<sup>49</sup>—the deliverances of the compositional machinery, therefore, would be both content-generating and *provisional*, awaiting ratification by competent judges.<sup>50</sup>

### 6.3.2. Some Difficulties

To date, Wright's suggestion has remained largely untapped.<sup>51</sup> I will incorporate elements from the suggestion into my own account, but let me first discuss some of the problems it gives rise to.

<sup>45</sup>George (1989). Wright had already expressed astonishment at the "scandalous" lack of interest shown by semanticists with respect to the RFC back in his (1980: 279). Since inaugurating the Minimalist Program, Chomsky has moved away from a view of I-language as a rule-based system, so Wright's original target has somewhat shifted. A reply of sorts to Wright's paper is in Chomsky (2000: 143). For his part, Chomsky (1986: 223ff.) seems to have taken the RFC rather seriously.

<sup>46</sup>I think it is no coincidence that two of the major proponents of Response-Dependence accounts were heavily involved in the debate on the theory of meaning that took place in the 1970s/1980s—see Wright's (1986a) and Johnston's (1988) contributions to the 'swan-song' symposium on that topic devoted to Schiffer (1987). One of the earliest discussions in this area is McDowell (1981b). Johnston (1989) is the first published statement of a Response-Dependence account of value properties. Wright (1992: fn. 16, 20) traces the genealogy of the idea.

<sup>47</sup>Wright (1992: 109).

<sup>48</sup>Wright (1989: 212).

<sup>49</sup>A distinction due, again, to Wright (1992: 110).

<sup>50</sup>I may be adding more to the view than was intended by Wright. Note that strictly speaking a view of this kind is more properly construed as *judgement-dependence* rather than response-dependence. See Holton (1992) for the distinction.

<sup>51</sup>Wright himself has given the topic another gentle tap in his (2007). The wider literature on Response-Dependence is intricate and fairly varied, but it has for the most part concentrated on discussion of the classic cases of colour and value properties. Casati and Tappolet (1998) gives a useful taster.

It is an essential part of the RD claim that there be a conceptual connection between our responses and *what it is* for the relevant property to obtain. It is, that is, *a priori* that our judgements be extension-determining with respect to the domain claimed to satisfy the RD constraint. And it is *constitutive* of the RD-norm attaching to an expression that our judgements be the *ultimate* arbiter of its exact profile.<sup>52</sup>

On the Chomskyan picture, however, speakers' judgements as to grammaticality and meaning are "merely data" in the empirical quest directed at the "real object of inquiry", namely, the "internal mechanisms that generate linguistics expressions and determine their sound and meaning".<sup>53</sup> An RD-account for such judgements would presumably have to insist that they are connected to the internal mechanisms as an *a priori* matter (to judge that *e* means *F* is what it is for *e* to mean *F*).

But now here's a first awkwardness: whose judgements are we extrapolating the *a priori* connection from? It is a familiar problem in linguistics that the judgements of the experts (presumably the embodiment of best opinion in these matters) are almost invariably *theory-laden*. Despite all effort, they inevitably reflect, that is, entrenched ideological commitment to a theory.

As for 'folk' opinion, well, it is an equally familiar fact that their judgement is just as often clouded by pragmatic considerations, unfamiliarity and processing limitations. In short, grammaticality judgements are affected by noise at both ends of the competence spectrum.<sup>54</sup>

It thus seems implausible that we could ever devise appropriate constraints both with regard to what would count as ideal judging conditions and as to who would qualify as an ideal judge.

The issue is further complicated by phenomena such as semantic saturation (the loss of meaningfulness commonly experienced after prolonged repetition of a string) and the already-discussed cases of verbal illusions.<sup>55</sup>

<sup>52</sup>Here's where Dummett's paradox presents itself again, should we choose to give an RD account of semantic properties.

<sup>53</sup>Chomsky (2009: 19).

<sup>54</sup>The point is made in e.g. Dąbrowska (2004: 2). Divergence of opinion among experts is equally well-attested. For just one example see Chomsky (1972a: 148).

<sup>55</sup>See ch. 2, fn. 181, p. 57. The interesting point about verbal illusions is that while they show some analogies to cases of visual illusions (the difficulty in shaking them off, for instance), there persists some disagreement as to whether they *are* genuine cases of illusions. Experts themselves disagree on the full extent of the phenomenon, that is. Here's another case for you to mull over: 'Many more people have been to New York than I have'—Smith (2006: 960), originally due to Paul

Examples of this sort suggest both that meanings may outrun (or at least: outfox) best opinion, and that our judgements are too inherently unstable to qualify as *a priori* connected to the putative RD-norms.<sup>56</sup> We seem unable, that is, to rely on the innocence of intuitions (whether tutored or untutored) and if we deferred to impossibly ideal judges we would simply be replacing the inscrutability of reference with *the inscrutability of deference*—hardly an improvement on our predicament.

Now, if we recall the EP, it seems clear that an RD-solution to that problem will have difficulties in providing *warrant* for a projection from past judgements to future verdicts on complex meanings *unless a background theory of some kind is in place*—mere past consensus will not *rationaly* ground either the future judgements themselves or our expectations concerning those judgements.

Arguably, the only promising way to secure judgement-stability under extension (of the language, or of the range of expressions being assessed) is to build an *a priori* assumption to that extent right into the account.<sup>57</sup>

But then, it is hard to see how an RD-account would substantially differ from the Platonistic framework it was trying to escape, since the good epistemic standing of our confidence in the authority of our judgements would still be stipulated by *fiat*.

Note also that with that *a priori* assumption in place, the RD-story would carry the further assumption that ideal speakers would return a *unique* judgement with respect to an expression-in-context, which in effect amounts to the Fregean assumption of *Eindeutigkeit* for NL expressions which had caused the problems in the first place.<sup>58</sup>

Pietroski.

<sup>56</sup>Further evidence also accrues from the literature on language change and the changing extension of the class of expressions that belong to the grammar proper—see ch. 2, fn. 119 p. 40.

<sup>57</sup>This difficulty is raised for the case of colour in the last section of Edwards (1998), from where I've borrowed and adapted the argument. Wright's (1992: 112) *Substantiality Condition* was meant to address this worry. However, no substantive account of its substantiality has yet been provided (Pettit (1999: §1) admirably sums up the difficulties). Wedgwood (1998) sketches an RD-account in terms of essences whereby RD-concepts would be such as to determine a unique property in virtue of facts about their essence (in the case of semantic properties, some version of Husserl's theory about the laws of meaning would need to be defended). Some of the claims made with respect to the principles of Universal Grammar are indeed claims about all possible languages. See e.g. Culicover (1997) and Newmeyer (1998, 2005). But these claims are far from undisputed. See e.g. Croft (2001), Hudson (2007) and Evans (2009).

<sup>58</sup>We cannot sensibly maintain that indeterminate content could give rise to (uniquely) determinate responses, that is.

### 6.3.3. Mind and Word

Despite these difficulties, it seems undeniable that our judgements do play at least *a* part (perhaps even a crucial one) in fixing (rather than merely tracking) linguistic structure. Incidentally, it was in any case rather curious that Wright should have read the RFC as posing a serious challenge to the Chomskyan project, for that project had a very ready answer to the Wittgensteinian claim that in following a rule we need have *nothing in mind*:<sup>59</sup> in fact, all that we need is *to have a certain kind of mind*.

That is to say, following rules in the (unthinking) manner that we do is but a reflection of “the way we are constructed”.<sup>60</sup> From the “biolinguistic” perspective,<sup>61</sup> nothing but “the inherent structure of our minds” could provide grounding for the normative hold linguistic rules have over our practices.<sup>62</sup> Language, that is, is not a formal object to be captured by rules of inference of the kind given in logic, but rather, it is a biological object, on a par with other modules in our perceptual system.<sup>63</sup> And in searching for the Wittgensteinian *Lebensformen* that alone can give signs their powers, we need look no further than the principles of Universal Grammar hard-wired into our brains.<sup>64</sup>

Clearly, a reply of this kind will not placate those hankering for normative constraints. We are back, after all, to the self-attribution of fairly brutish dispositions to humour the dictates of our internal representations. And what is there to distinguish *that* from mere habits?

Well, here’s a possible suggestion: the internal representations posited by linguistic theories do express normative constraints *to the extent that they reflect deep facts about conceptual structure*.<sup>65</sup>

The idea would be that whatever ‘naturalness’ is contained in the norms that we find compelled to encapsulate in our judgements regarding semantic properties derives its good standing from the fact

<sup>59</sup>As Wright (2007: 486) “dangerously” phrased it.

<sup>60</sup>That is the blunt answer to the RFC (and Wright’s anti-Chomsky use of them) contained in Chomsky (2000: 143). Similarly, for Hinzen (2007: 27) the normative force of linguistic structure is due to structural facts about our minds.

<sup>61</sup>Chomsky (2009: 13).

<sup>62</sup>Hinzen (2006: 147-48).

<sup>63</sup>Hinzen (2007: 154-55) remarks that linguistic structure is much more constrained than logic. There is no way to derive the structuring principles of language on purely logical grounds, we are told.

<sup>64</sup>Chomsky (1986: 232).

<sup>65</sup>There will be a certain feeling of *déjà lu* for the next page or so for those readers who remember our discussion from §2.4.

that those judgements *are* tracking ‘eligible referents’ of some sort or other.<sup>66</sup>

There is thus a metaphysical (and not purely biological) underpinning to those norms—the ‘right’ sorts of judgement are those that stay sufficiently close to the order of things, an order that our concepts faithfully mirror. On this view, the RD-account had shifted the balance too far on the side of the judging subject (and, in the Chomskyan approach, her mental structures): the genuine normative constraints that we are after should rather be located in the way in which we properly model reality by our use of language.

#### 6.3.3.1. La Règle du Jeu

Now, the thought is indeed reassuring, for it seems to reconcile intuitions about that traditional philosophical pursuit, the *a priori* investigation of conceptual structure, with the comforts of having evolutionary science on your side (the language faculty has developed in the way it has because of a sound, and indeed sane, connection with the facts out there).<sup>67</sup>

But we are not out of the woods yet, because there are a couple of remaining wrinkles in the story.

Firstly, it has long been a favourite contention of Chomsky’s that what is significant about the language faculty is its independence from other cognitive faculties: syntax is autonomous, is the war-cry.<sup>68</sup> And indeed Chomsky’s understanding of the CC departs from the Fregean tradition prevalent in philosophy of language: for him, the remarkable feature of language is its ability to *detach* itself from context; it is the *adaptive* character of the output of the language faculty that is taken to be the genuinely creative aspect in need of explanation.<sup>69</sup>

By contrast, the philosopher’s obsession is with how to bridge the gap between (possibly skeletal) linguistic expressions and the full determination of the truth-conditions standardly attached to them.<sup>70</sup>

<sup>66</sup>The notion comes from Lewis (1983: 479).

<sup>67</sup>Note however that Chomsky (1975b: 59) has been careful to keep evolutionary issues well separate from questions as to the intrinsic properties of linguistic structures.

<sup>68</sup>See e.g. Chomsky (1957: ch. 2), (1986: xxvi) and (1975b: 55).

<sup>69</sup>See Chomsky (1966: ch. 2) and the discussion in McGilvray (2001). There are, rather surprisingly, points of contact here with Derrida (1982: 317).

<sup>70</sup>As is familiar, Chomsky has always been distrustful of the very notion of truth-conditions preferring that of ‘truth-indications’. Quite how that squares up with his insistence that a grammar is about sound-meaning pairings is an intriguing question. My proposal in the final section will actually be very close indeed to his views on this matter.

Secondly, the linguist's preoccupation is usually with what language does *not* allow you to do,<sup>71</sup> whereas the philosopher's is with the unbounded possibilities that language *can* encompass, the indefinite number of ways there are for leaves to *be* green, for instance.<sup>72</sup>

The problem with locating language's normativity (and its authority over us) in facts about our minds, possibly buttressed by deeper metaphysical underpinnings, is thus two-fold: a reply of this kind is largely naturalistic, whereas the proper construal of Wittgenstein's RFC requires seeing it as posing an *anthropological* question, the question of what it is about our practices that makes *us* rational, and not that of what it is about our *brains* being wired along certain principles that might have *served* us well.

Moreover, the hope of anchoring language on conceptual structure faces the further issue that we seem forced to try to map a realm, that of our concepts, populated by entities that are constitutively *unambiguous* (for *that* is what alone can identify them—nothing short of sharply defined possession conditions will do in pinning down a concept as the concept it is), by means of entities, our signs, that are (just as constitutively) *ambiguous* (or rather: *polysemic*).

The traditional answer inherited from the Frege-Russell tradition is that we bridge the gap by making language rigid enough to mirror the putative stability of our concepts—we idealise away the rough edges, we capture the hard kernel of our concepts by appropriate regimentation of our language; we translate the ordinary language of the vernacular into the extraordinary language of the logician.

On this view, then, our sensitivity to rules-as-reasons is best construed as the ability to track the sharp boundaries of our concepts beneath the confusing looseness of our signs.<sup>73</sup> And indeed, as is familiar, the idea behind the Fregean notion of *Sinn* was that our words *could* express a conceptual content precise enough to map *our* route to their referents within uniqueness.

But that was precisely the picture that the RFC had put under pressure: we cannot construe our rationality *qua* language users as the (uncanny) ability to 'read-off' *which* Fregean meaning-function is associated with a (potentially polysemic) expression and track its deliverances-in-context (on the whole) with accomplished (if defea-

<sup>71</sup>See for instance Newmeyer (2005: ch. 1).

<sup>72</sup>The concern, as already mentioned, is with what Sainsbury (2001: 201) has called the *Travis Effect*, the seemingly ineradicable underspecification of all our sublunary predicates.

<sup>73</sup>As e.g. Miller (1998: 179) spells it out, a Platonistic conception of semantic properties would have it that expressions have correctness conditions attached to them such that our competence consists in the ability to track whether or not the property is instantiated in a given context of use.

sible) efficiency.<sup>74</sup>

The epistemology of such functions is, it seems, incoherent (we could *always* raise doubts as to its efficacy). And it is equally unclear what normative hold they could possibly have on us, since we would always doubt that we had implemented their instructions appropriately.<sup>75</sup>

The rules of *our* language game, that is, are (or ought to be) first and foremost rules about its *jeu*, about the essential *instability* of the sign, the play (the give-and-take) that ties concepts to their expression as *both* signs and concepts adapt to contexts that are *always* different in *significant* respects.<sup>76</sup> Neither concepts nor words are as sharp as the semanticist picture assumes, that is.<sup>77</sup>

So, while I do incorporate RD-elements in my account, I am instead discounting both the sort of purely naturalistic answer standardly given within the Chomskyan framework and the view that would anchor normative constraints purely on the equally natural eligibility of a certain range of referential anchors for our conceptual scheme.<sup>78</sup>

#### 6.4. Background, Blindsight, and Authority: Generalising Indexical Thought

It is now time, at last, for me to sketch my view on these matters. To introduce my proposal, let me recall Perry's (1977: 16) point that self-locating beliefs do not consist in believing a Fregean thought.

I'm proposing that *all* thoughts are best characterised as indexical (i.e. sharply non-Fregean) in this sense, i.e. they all *rely on* (note: I am not claiming that they *contain*) irreducibly perspectival components of various degrees of strength, no matter how 'timeless' those thoughts may look on the surface—even mathematical thought is perspectival, to the extent that it is relative to (and, crucially, *parasitic on*) certain background conditions being operative in a discourse (that, for instance, signs mean the same in all their occurrences).<sup>79</sup>

<sup>74</sup>See also Williamson (1994: 281, fn. 25) for criticism of this view.

<sup>75</sup>Travis (2006b) is rather convincing on this point.

<sup>76</sup>The discussion in Glendinning (1998: ch. 5, 6) is particularly illuminating, and it informs, along with Edwards (1990), much (but not all) of my thinking in this area.

<sup>77</sup>As I discussed already in chapter 2, anchoring semantic judgement to conceptual structure weakens the PoC/CC explanatory connection because of the demonstrable diachronic and synchronic fluidity of our conceptual schemes.

<sup>78</sup>I have given reasons for distrusting both views back in chapter 2 and 3.

<sup>79</sup>Here I was tempted to talk of thoughts as *containing* indexical elements and explain that away as mere rhetoric. The important point, regardless of how I formulate it, is that I am not maintaining



None of them ever expresses (or is equivalent to) a Fregean thought, for such thoughts would be literally *unthinkable*: Frege's *Drittes Reich* is not just inaccessible; it is empty.

Accordingly, grasp of sense ought to be seen as grasp of entities that depend (for their achieving whatever determinacy they can achieve) on irreducible perspectival elements which themselves resist full articulation.<sup>80</sup> This is of course because of the anti-CET worries: just as facts about one's self-location are not expressible in a language (*any* language), so background-driven facts about content sharpenings are similarly not expressible in language.

I will therefore argue that, strange as it may seem, we can secure determinacy of truth-value in the absence of truth-conditions semantically conceived.<sup>81</sup>

What will guarantee determinacy will be *agreement in judgements*, but this will be only *ex post facto* determinacy.<sup>82</sup> Neither the facts triggering that agreement, nor the facts about the content agreed upon, however, can be captured formally—in the latter case, because *there are no such facts* (the determinacy is *posited*, not “real”: thinking is

that egocentric elements are *part* of what we think. What I later call ‘enabling conditions’ make it possible for us to have a world-directed thought—that’s the sense in which our thoughts are indexical. But properly speaking those conditions are never part of our thoughts (*nor* of our theory). Our thoughts (and our theories) remain (frighteningly) minimal.

<sup>80</sup>Two things: first, what we grasp are minimal entities (purely internal representations); they depend on perspectival components to *approximate* determinacy-in-context. Secondly, note that this generalisation is not intended to make all sentences essentially indexical, or else we’d lose a good distinction between saying ‘I am cold’ and ‘Walter is cold’. As suggested by e.g. Dancy (2004: 196–97) and Schiffer (2003: 100), here it may be not entirely unhelpful to think of grasp of sense as something akin to knowledge by acquaintance, for knowledge of language is more akin to knowing-how than to knowing a bunch of (propositionally stateable) rules for use. The claim that knowledge of language is an ability has of course been made often. What is distinctive about my position is that even those aspects of competence that one is tempted to characterise in terms of propositional knowledge require substantive amounts of knowledge by acquaintance to be implementable. To the extent that we can make sense of rules of language, they will be essentially *incomplete*, they will be functions that are partial in a very strong sense (no value is defined until our judgements are incorporated into the workings of the function *in situ*). Evidence from language change shows that there is no reason to discount a view whereby settled facts about syntax are nothing but long-standing judgements congealed in the very fabric of language. See Newmeyer (1998: ch. 5) for a contrary view.

<sup>81</sup>To echo Wright’s (2002b: 421) remarks in a different context. Unlike him, I think this is a live possibility.

<sup>82</sup>As will become obvious, there are numerous points of contact here with the work of a number of post-Wittgenstenians, e.g. Stanley Cavell, Charles Travis, and Jonathan Dancy. I’m really not sure where to place John Searle in all of this. At times he’s very radical in this respect, at other times he sides with what I would call semanticist positions. For the radical aspect of his view, see e.g. Searle (1994: 640). For the conservative aspect see Searle (1994: 660), where we are told that the meaning of the *type* can be *insulated* from the influence of the changing Background.

not, and could not be, the manipulation of fully determinate entities).

If I am right, it follows that logic does not deal with fully determinate entities either. The relata of the consequence relation, that is, are not fully-blown propositional entities but rather something much more minimal—not in the sense that we reason about *thought-kernels* amenable to expansion-in-context; we *always* reason with *essentially* minimal entities.

Accordingly, and to answer a question that I left dangling from the last chapter, consequence is situated in the sense that for  $\varphi$  to follow from  $\Gamma$  we require a pre-existing agreement in judgement regarding the reasonableness (or otherwise) of investing the expressions involved with a given understanding. Neither the required understanding nor the agreement concerning it are however susceptible to formal treatment.<sup>83</sup>

Let me now detail some more reasons that might support a view of the kind I'm defending.

#### 6.4.1. Four Ways of Mapping Language to Thought

As we saw, behind the semanticist's insistence on determinacy lies the presumption that *the very act of thinking a thought G can only be individuated by the univocality of that thought*. The idea is: while signs may be ambiguous, thoughts *cannot* be so—they are *constitutively* unambiguous, that is, fully specified, without gaps of any sort (that's what thoughts are; they *couldn't* be otherwise). There may be room for variation in the way we all home in on the same thought. But it is the stability of the target content that guarantees interpersonal agreement, and indeed interpersonal communication *tout court*.

I think this picture is deeply misguided. To show why it is so, I need to sketch what seem to be the only conceivable ways for the connection between thought and language to hold.

It seems as if we have four broad possibilities to begin with (where 'D' and 'I' stand for 'determinate' and 'indeterminate' respectively):

The fourth option seems hardly plausible, so I won't consider it here.<sup>84</sup> The first option seems to express most people's view of

<sup>83</sup> As Travis (1996: 102) and (2008: 6) has argued, there is no way to have a logic of understandings (as functions that "extract content from circumstances") for there is no fixed list of understandings. But we cannot require either, as Travis (2009) instead suggested, that logic incorporate additional assumption rules dictating that only formulas satisfying certain conditions (e.g. that their meaning be made uniformly determinate by antecedent understandings) would be admissible for purposes of proof (the reasons are detailed in Williamson (2009) and have to do mostly with the crippling loss of the principle of uniform substitutivity).

<sup>84</sup> That's not quite right, actually. For there is a way of reading my proposal as being committed

Thought	D	D	I	I
Language	I	D	I	D

the matter, if language is taken to be NL. The second option is the Fregean (and indeed the logician's) view of what a formal language can achieve: matching thought's determinacy blow by blow, as it were. Bridging the gap between position 1 and 2 is what semanticists (and RT- and TCP-pragmaticists) are trying to secure: making NL as precise as thought, by some means or other.<sup>85</sup> The third option is the one genuinely radical pragmaticists like e.g. Travis favour.

My own view is a kind of hybrid between 2 and 3—just avert your incredulous stare until the next section, if you please. Before I turn to that, let me first review the possible ways of implementing the transition between 1 and 2.

So, position 1 has it, with Frege, that thoughts are determinate (TD), univocal, constitutively unambiguous, fully specific entities; in contrast to that, NL is instead endemically indeterminate, ubiquitously equivocal and so forth.

How can we bridge the gap between the two layers? How, that is, can we fix semantic content in such a way as to map it to precisely determined thought content (and thus move from position 1 to 2)? Well, a first option is to argue that formalisation provides an intermediate step that allows us to map underspecified sentences to fully specified thoughts. This could be done *i*) the way Montague did, or *ii*) by means of some form of indexicalism (either LF- or Clause-indexicalism, as we have seen in chapter 3 and 4). Or *iii*) we could instead argue that it is pragmatic means that get us from the schematic entities outputted by the semantic module to fully specified thoughts (as formal pragmaticists have argued).

to precisely this position: that language is *disquotationally* determinate while thought is indeterminate. My view however is that thought too is disquotational, in the sense to be made precise in the text. In any case, the notion of determinacy in the literature is usually assumed to be something like: 'questions regarding truth-conditions are univocally settled by the rules associated with the expression.' And that's a notion that I reject for both thought and language.

<sup>85</sup>As I argued in chapter 3, while pragmaticists often insist that *semantic* content is underspecified, they still allow that it can be *completed-in-context*: the final outcome of the interpretation process is still some sort of Fregean thought. And indeed, with the exception of Travis, their anti-semanticist argument *relies* on the alleged contrast between fully saturated thoughts and the radically unsaturated character of *all* linguistic content.

As I've indicated in previous chapters, I think the first two views are unstable, to the extent that they try to get language to match the supposed determinacy of thoughts in terms of *expressible* determinacy—we somehow stipulate the identity conditions for a thought (either in terms of possession conditions or of clauses formulated in a semi-formal language) and then proceed to flesh out semantic clauses of various kinds to secure the same level of determinacy for our signs as that enjoyed by our thoughts, either via an intermediate translational stage (the disambiguating language mediating between NL and thought) or via the positing of hidden semantic enrichment at LF. But clearly, for by now familiar reasons, it is obscure why the *description* (and grasp) of those conditions should be immune to indeterminacy doubts. The third view is equally unstable, again, for the reasons I already discussed in chapter 3.

A fourth option is to admit that *iv*) determinacy by purely semantic means is unattainable. Determinacy is nevertheless ensured by a convergence in understanding by “the great majority of hearers competent in the language” Dummett (2004: 7). And even though facts about that convergence are not captured by rule-based accounts, the fully Fregean status of our thoughts is unimpeached.<sup>86</sup>

Now, although this position is a definite improvement over the previous three (for it acknowledges the constitutive shortcomings of signs and rules with regard to the fixing of content), it is still, to my mind at least, unsatisfactory, because on the one hand it does admit the gravity of our predicament (the inability of *our* language to secure content individuation: the inability of *unaided* rule-based accounts to fix determinacy) whilst still insisting on the ultimate determinacy of thought (and understanding).<sup>87</sup>

A burning question remains, however: What *evidence* do we have to believe that thought itself *is* determinate, if we have no better *evidence* for it than the portentous convergence in judgement as to *which* understanding is reasonable in a given context?

In fact, the very idea that we *do* home in on a *single* understanding is just as incoherent as the Relevance Theory position I critiqued in chapter 3, for it assumes that there is a range of candidate interpretations available for the taking that are *themselves* determinate, whilst

<sup>86</sup>It looks to me as if McGinn's (1989: 199) position is along similar lines: “models realize content only in as much as they are embedded in a certain background of goals, behavioural propensities and a network of causally related states”. The final step to full Fregean determinacy is the one I want to resist.

<sup>87</sup>Moreover, the very same determinacy is claimed to be reachable *by the theorist in her semantic clauses*.

also insisting that *we can* all *converge* on that mysteriously elusive target from starting points that are, *ex hypothesi*, deeply indeterminate.

The point is exactly the same as the one raised in connection with the CET worries: *what* we agree upon in a context can *not* be a determinate thought, for if we could agree that *it* is, we would not have had a problem in the first place (there can be no determinacy for the candidate items if there isn't determinacy for the sentence we started from—can you sharpen a blunt instrument with a blunt file?).<sup>88</sup>

More generally: if convergence in judgements regarding how an expression *ought to be* understood is a *pre-condition* on determinacy, it is not clear to me why the existence (or is that the *appearance*?) of such a convergence should be taken to entail the existence of determinacy at the level of thought (there's an unwarranted slide from a necessity claim to a sufficiency one: convergence *may* be necessary for determinacy, but it doesn't follow it gives sufficient reason to believe there is such a thing as determinacy of content). After all, transcendental arguments do not have much of a track record in establishing existence claims and the implicit *modus tollens* step that supposedly sustain the transcendental move (i.e. : or else we would have no practice to speak of) can enjoy no privileged status over the *modus ponens* step I'll be going for.

A fifth way of implementing TD (with substantial elements of overlap with Dummett's idea) is *v*) the one defended in various versions of situation theory.<sup>89</sup> On this view, semantic content is relatively thin, but by adding background/contextual components to it we obtain an *Austinian proposition* which is in effect a complete Fregean thought.<sup>90</sup>

Both these last two strategies in effect propound a *composite* view of content: content is factored out into a semantic component integrated by a background component. Determinacy requires both components. Both *iv*) and *v*), that is, allow that background conditions are not specifiable in the way the semanticist wants, but nevertheless maintain that those factors somehow add up to a fully determinate piece of content.

Predictably, I do not think the Situation Theoretical view is tenable either. It just buys the eternalist a little more time, but the ultimate problems still remain; they just arise at a different level in the posited machinery—very roughly, the chief difficulty is how to match situation and *lekton* (i.e. the level of semantically expressed content which

<sup>88</sup>To repeat: the difficulty here is that the move from surface indeterminacy to hidden determinacy is supposed to be carried out *using exactly the same sort of means*, i.e. content-bearing entities.

<sup>89</sup>Barwise (1989), Barwise and Perry (1983), Recanati (2007a: 46-7).

<sup>90</sup>See Recanati (2007a: 46).

is, again roughly, a propositional function from situation to content) *uniquely*.<sup>91</sup>

A final and sixth way is, roughly, a version of the epistemicist view: there is determinacy in thought and content, and it's only our ignorance (rather than just constitutive facts about signs) that blocks our access to facts about extension.<sup>92</sup> NL is a blunt instrument but mercifully logic (classical logic of course, for underneath the non-classical dust the world is resolutely classical) provides a regulative ideal that lays down clear stipulations as to what counts as inference—and below it all, the extensional boundaries of our thoughts are as sharp as we may wish them to be.

Clearly, this is a radical form of LIR (as is explicit in e.g. Williamson 1994, 2000, 2005) and again its main motivation is a *modus tollens* move to save logic, science, and realism (a *troika* that, apparently, we cannot do without) from the radical contextualist rabble.<sup>93</sup> For my purposes this is just a (more rabid) version of *ii*) above and hence I deem it just as (if not more) unpalatable.

### 6.5. Radical Minimalism (Without Boundaries)

We are left with option 3, then, the idea, that is, that both language and thought are indeterminate—whatever sharpness we think (some at least of) our concepts may have is merely in the eye of the beholder, so to speak (underneath, it's all a bit of a muddle, really).

On the face of it, the view might seem a little curious, for we seem to be saying that thoughts are indeterminate but nevertheless language,

<sup>91</sup>Note that Austinian propositions include both semantic content and circumstance. Its typical statement is something of the form  $s \models \sigma$ , where  $s$  is a situation and  $\sigma$  an *infor*, a propositional function making a claim about a situation. They are in effect *situated Fregean thoughts* and have absolute truth values—see Barwise (1989: 273). Once we have Austinian propositions, we've therefore gone fully back to classical logic and indeed the consequence relation in situation theory is defined in completely classical terms. See Seligman and Moss (1997: 283, 295). Situational information is used to account for the direct content of the attitudes, but once circumstance is included the story joins up the classical account and truth is readmitted on the scene. Situation theory, however, suffers from the Problem of Specificity, that is, how to specify the connection between the information content of a statement and *the* situation that supports it. The problem for traditional semantics was how to connect underspecified semantic content to truth conditions; that for situation theory is how to connect the situation to the Austinian propositions that it supports.

<sup>92</sup>As Quine (1960: 193) put it with his trademark confidence: “vagueness, ambiguity, fugacity of reference, are traits of verbal forms and do not extend to the objects referred to”. I'm not sure of two things here: whether this tallies with Quine's ontological views as expressed elsewhere, and whether we have good reason to dismiss ontic indeterminacy so insouciantly.

<sup>93</sup>For this move, see especially Williamson (2009: 377-384).

itself a largely indeterminate affair, somehow succeeds in determinately referring to indeterminate concepts.

I say “that’s round”, and I thus determinately express a concept (there is no doubt about *which* concept I intend to refer to) which is itself indeterminate. But if the concept is itself indeterminate, how do I know (how does *anyone* know) *that* I’ve hit the *intensional* target, let alone that I have thereby hit its (underspecified) extension too?<sup>94</sup>

Here’s a suggestion. Think of a concept-word as expressing something *very* minimal (*mutatis mutandis*, similar considerations will apply to all expressions). Don’t think of it as an abstraction from *all* contexts of use (the minimal core that all uses of that concept-word have in common).<sup>95</sup>

Think of it as a minimal concept defined *in terms of its relations to other concepts* (e.g. ‘not square’) as well as in terms of its connection with a range of *prototypes* (paradigm cases of e.g. roundness that you’ve been trained to recognise as such)—we need this second clause to go beyond a purely structuralist view of properties (in the post-Saussurian sense). The concept—and the expression—will be individuated in virtue of those relations.<sup>96</sup>

Crucially, don’t think of it as something defined in terms of its intrinsic properties (sense is not something that gets you the ‘cognitive fix’ onto a referent by means of necessary and sufficient conditions for the applicability of an expression). Don’t think of it in terms of Lewisian natural eligibility either (whatever eligibility there might be is anything but natural).

Think of it, rather, as something defined in terms of its extrinsic, relational properties, properties that are themselves largely open-ended and provisional (they are indeed broadly criterial, judgement-based).<sup>97</sup>

<sup>94</sup>Szabó (2000: 42-3) does suggest that we could have precisely specified rules picking out indeterminate entities. I struggle to make sense of this proposal.

<sup>95</sup>An idea of this kind seems behind the classic semantic minimalism of Soames (2002), Borg (2004) and Cappelen and Lepore (2005a). For (very good) reasons why it is a bad idea see e.g. Taylor (1989: ch. 8) and Travis (2000: ch. 1, 3, 7).

<sup>96</sup>This seems to force a holism about meanings that is alleged (see e.g. the classic attack in Fodor 1987: ch. 3) to make meaning a mystery (there is no knowing a single meaning without knowing the entire language). Answering this charge as applied to RCM would take more space than I have left. You will just have to take my word for it.

<sup>97</sup>On the criterial conception, see again Wright (1978, 1982). Note that my conception is *weakly criterial*, in the sense that while content is indeed fixed by criterial satisfaction, there is no mapping from criteria to their potential satisfiers. One more word about judgements: on the Chomskyan picture, speakers’ judgements are *evidence* (or rather one piece of data among many) on which we build our theory of competence. By claiming that semantic properties are (partly) judgement-based, I seem to be reversing that picture, since I seem to be *explaining* semantic judgements in terms of

Like the objects studied in e.g. Category Theory, then, semantic objects (and the corresponding concepts) are thus individuated in terms of the relations holding between them, rather than in terms of their relations to e.g. full-blooded propositions, facts, or what have you.<sup>98</sup>

Shockingly, again, they are objects *depleted of nearly all content*. It's the mapping between them that will establish their ability to carry content in context.<sup>99</sup> And those mappings are fixed by their embeddings in various linguistic and extra-linguistic contexts.

The further point is that on this proposal rationality (in the sense of: rational use of language and indeed: rational entertaining of thoughts) is not a question of *just* obeying rules (i.e. of conforming to their dictates, of being disposed to acknowledge the guiding power of those rules) but rather a question of having the ability to deploy essentially skeletal concepts in ways that are appropriate to the circumstances *as judged to be so by competent speakers*.<sup>100</sup>

Now, one key part of my proposal is that we *cannot* go beyond the purely homophonic clauses in *any* robust sense.<sup>101</sup> If you are still in the thrall of semanticism *and* pragmaticism, my claim here is that no *further* step is to be taken. There is no *completion* in the offing. There is no further (and finer) carving for content available.

The fact is, minimal content, in the sense sketched above, cannot be *sharpened*. It is as sharp as it'll ever get. Indeed, it is not the *sort of thing* that is amenable to sharpenings.

And there is no hidden sharpening in the background beyond our epistemic reach either—it's not that background conditions, immersion in Wittgensteinian 'forms of life' or the McDowellian partaking in the stable deliverances of Aristotelian second nature could play a

semantic judgements. I think the circularity is only apparent and no more vicious than is the case with standard accounts of rationality and agenthood (characterising normative notions *is* a circular enterprise: *that's what it means* for something to be normative).

<sup>98</sup>Here's another analogy: consider the Frege-Hilbert controversy; Frege (1880/81: 48) argued that we could not speak of *the* parallel axiom "as if it was the same in every special geometry". Only the wording is the same, he explained: "the thought content is different in every different geometry"—that's CET again. I side with Hilbert's reply (p. 51): "a concept can be fixed logically only by its relations to other concepts".

<sup>99</sup>I take this view of the Category Theory approach to object individuation from Lawvere and Rosebrugh (2003: ix). Relational views of meaning are also operative in Croft (2001), Goldberg (1995, 2006), Hudson (2007) and Fine (2007). Inferential properties do play a major part in establishing an expression's cognitive profile. RCM is however more radically anti-representationalist than Brandom (1994, 2000, 2008) for I discount the possibility of defining content in terms of incompatibilities—I take Wright (2001a: 350) to have disclosed the hidden representationalist commitments of that notion.

<sup>100</sup>If one complains that this leaves the grounding of those judgements obscure, I reply: quite so.

<sup>101</sup>In that respect, but in that respect alone, I share Segal's (2000) slim notion of content.



role (unknownst to us) in making content *determinate*: such things can do no more than merely *force* parochial agreement; they leave content absolutely untouched in its stubborn minimality.<sup>102</sup>

On my view, then, content remains firmly sub-propositional (in the traditional sense) *all the way through*.<sup>103</sup>

Properly speaking, then, sentence-type semantics is no semantics at all.<sup>104</sup> And competence is not competence with abstract types, for the very simple reason that there is no such thing as the null-context, or the entertaining of types in abstraction from *all* contexts. We *always* think in a context (even when we write books about semantics), we project ourselves in a context, and we always entertain sentence-tokens in a given (if possibly highly etiolated) context. In other words, competence is grasp of *prototypical* tokens, not of fully de-contextualised types.<sup>105</sup>

How do we succeed in referring to *each of the ways for things to be round*, then? Well, here's where I also part company with the composite views of content I clustered under *iv*) and *v*) above. For, as I've just said, I do not think that content is *made* determinate by some kind of compounding of semantics and situatedness.

Situatedness, that is, does not *add* sharpness to our concepts (or expressions). It just makes it *appropriate* to *treat* concepts (and expressions) *as if* they were sharp, given the purposes and norms operative within that situation.

What situatedness does provide is *enabling conditions* for certain judgements about the appropriateness or otherwise of e.g. asserting a sentence in a given context.<sup>106</sup> But these conditions are neither

<sup>102</sup>That's where I think I depart from Searle's view.

<sup>103</sup>Relatedly, I also reject the claim typically made by minimalists that pragmatics does not intrude on semantic content, for I happen to think both that *settled* semantic content is *shaped* by pragmatic concerns that have seeped into the very fabric of language and that without *continuing* pragmatic input there is no *individuation* of content at all. The compositional machinery however operates only on *radically* minimal semantic content.

<sup>104</sup>Paraphrasing Tennyson, such a semantics would indeed be so careful of the type and so careless of the single token. There's however a contrary argument (which I reject) to the extent that our competence is competence with types, and that there could be no semantics of tokens because unless we can identify a token as the token of a certain type, there's no individuation of content-in-context. See e.g. Bach (1994a: 85-86).

<sup>105</sup>Again, I should address Fodor's (1998: ch. 5) anti-prototype argument here, but lack of space prevents me from doing so. At any rate, my view of prototypes differs from the one targeted by Fodor.

<sup>106</sup>Talk of enabling conditions may invite comparison with discussions of content externalism. I would not discourage that. If you want to think of enabling conditions as the obtaining of certain causal relations between our words and the world, I won't object to that either. What I resist is the claim that those relations are *in any non-trivial way* determined by our words (or theories).

amenable to theoretical treatment nor do they succeed in CET-fixing content (not even in ways beyond our ken).<sup>107</sup>

Mine, in short, is a kind of reverse Humeanism about semantics (and thought): it's not our mind spreading over the world; it's not even the world spreading onto our words. It's rather that we simply get our projections thrown back at us in our words.<sup>108</sup> And at no point in the procedure is there determinacy.

To see the contrast with the view from which we started, consider once again Frege's (1906: 315/384) take on the matter: "The word 'interpretation' [*Deutung*] is objectionable, for when properly expressed, a thought leaves no room for different interpretations." Given the semanticist framework, there is *no* alternative but to take *that* view: once we reach *Bedeutung*, we find its *Deutung* built right into it.<sup>109</sup>

Determinacy is thus the absence, indeed *the exclusion of alternatives* (and the process of interpretation is therefore seen as the decremental shedding of discarded alternatives until a unique last candidate is left standing).

I hope I've convinced (or perhaps reminded) you of the reasons why the Fregean position is mistaken (it paints us into a very tight epistemic corner indeed).

Our problem seemed to be how to *express* unambiguous thoughts by guesswork-ridden means (and in response one gestured impatiently at the limitative features of NL that logic *was specifically designed to redress*).<sup>110</sup>

The real obstacle, however, resided in the mistaken conception of the stuff we were trying to express in the first place. It is the mistaken

Barwise (1989: 66-67) points out that there are some components of circumstances that are both *unarticulated* and *non-constituents*; that is, they are neither part of the syntax (hidden or overt) nor of the context as described/evoked by the propositional content. They are in effect Searlean background conditions, or rather background *conventions*. Barwise notes correctly that the dividing line between when a certain piece of information goes into the background or into the propositional content is not a sharp one. It varies across time and contexts.

<sup>107</sup>Strawson (1998: 24) had concluded that contextual influence on content cannot be reduced to "precisely statable rules". My point is that it cannot be reduced to rules *at all*, for in a very good sense there is *no* influence of context on content (only on truth value). RCM is a strongly invariantist conception of content, that is.

<sup>108</sup>I reinforce and widen *Investigations* §104, that is: the objects of our thought are *methods of representation*, whether or not they are *actually* world-directed. And our thoughts can have no more determinacy than those methods afford. However, RCM is no neo-Lockean ideationism.

<sup>109</sup>The play on words here is lame: we are supposed to talk about *Sinn* here. But think of *Bedeutung* as the semanticist's notion of semantic content.

<sup>110</sup>I cannot resist the (mischievous) temptation to invoke e.g. Rorty (1978): what I have been (surreptitiously) urging throughout this dissertation is that, yes, philosophy *is* a kind of writing.

view of (perfect) thoughts that generated a mistaken view of (imperfect) language.

In response to our original difficulty, it should now be clear that I do not accept what I take to be a grave *non sequitur* which e.g. Dummett (2004: 8) is guilty of when he moves from the Travis-like observation that (something like) the CET considerations force us “to consider statements, not as mere concatenations of words, but as subject to particular ways of understanding them” to the Frege-friendly conclusion (introduced via a wholly misguided ‘that is’) that *those* ways express “specific thoughts and propositions”.

This is the conclusion I’ve spent the previous chapters trying to undermine. It is shared by all positions on the table (bar Travis’), and it is the idea that the end result of interpretation is, if not a specific thought outright, something (a Kaplanian character perhaps in need of integration via a logic of ‘understandings’) that nevertheless can take us, given certain background conditions in place, to a specific thought.

What divides the opposing accounts which I have rehearsed until now is disagreement over the ways (and means) by which we get to that final stage in the interpretive process. All accounts however agree on the contention that we get *there* all right, that we do achieve determinacy, one way or another. Or else, we’re invariably told, how could we possibly understand each other?

*This* is what I want to resist and the thing is: if *their* picture were accurate, we could *not* understand each other (and logic could not be the science of reasoning, and semantics could impose no normative guidance on use).<sup>111</sup>

Given the facts about *our* language, determinacy in the hugely robust sense assumed by all participants in the dispute would be an *obstacle* to understanding. Fully determinate thoughts (or partially determinate thoughts made precise by grasp of situatedness) would be computationally opaque and thus beyond our cognitive reach.

Similarly, Fregean meaning functions that would unerringly determine the content of our expressions would transcend our cognitive powers—there could be no agreement on *their* deliverances.<sup>112</sup>

And this is also why a move to a character-level logic (and seman-

<sup>111</sup>The account of communication in Bilgrami (1992) was meant to address this difficulty in the semanticist account. I do not think it succeeds but I have no space to argue for this here.

<sup>112</sup>The point is made repeatedly in Travis’ work, see for instance Travis (2006b: 134–39). As Sainsbury (2001: 203) notes, Travis-meaning is both unspecific and interest-relative. More generally, Fregean meaning functions would fall foul of Peacocke’s Integration Challenge by supposing (to paraphrase Johnston 1989: 171) that the demands of semantics could be thoroughly independent of our ability to respond to them.

tics) cannot solve our difficulty.<sup>113</sup> For the transition from character to content cannot be disciplined with the required degree of formality either—again, all we’re doing is shifting the CET problem at different joints in the machinery; the problematic assumption that *at some node* there is a uniquely determined instruction (or set thereof) remains in place.<sup>114</sup>

The upshot then is that there can be *no more* precision to our thoughts than what is secured by our agreements-in-judgement (and openness to negotiation under reflective scrutiny).<sup>115</sup>

#### 6.5.1. A Wrong Turn?

So, do I think that my RCM is the way forward, indeed the way out of all the worries that I have raised up to now? Or couldn’t one rather say that in fact RCM is the fruit of yet another deep confusion between competence and performance, in the sense that it overlooks the possibility that the semanticist might simply say that “cognitive processes can be sensitive to available evidence without having to represent all of it explicitly”?<sup>116</sup>

This latter thought is basically on a par with one obvious way of reading the McGinn idea I sketched at the end of the last chapter: the fact that linguistic representation ought to be receptive to contextual factors does not entail that it has to incorporate those factors into its own content.

Sure, our use of language is receptive to content-fixing contextual elements that elude representation, the objection goes. It does not follow from that consideration that content itself cannot ever be determinate—and indeed (and moreover), RCM does get the direction of fit (as well as the explanatory priority) back to front: we best explain agreement in judgement as *recognition* of an antecedent fit between possible sharpenings of our expressions and worldly facts.

The charge would then be that I’ve simply badly misread the

<sup>113</sup>Of the kind sketched in e.g. Williamson’s (2009) response to Travis.

<sup>114</sup>Suppose we give a logic of characters: the problem then becomes how we can ensure that the move from type to token preserves validity. We need a guarantee, that is, that whatever features of an expression we have identified at the type level is preserved when we move to its tokenings. Dever (2006: 655) points out some difficulties with reference to the compositionality of characters. See also Schiffer (2003: 120) for the remark that there are sentence-types whose character does not determine content (I take Schiffer to be saying that for some sentence-types, the characters we can formulate are too imprecise to determine content).

<sup>115</sup>This is where the connection with Scanlon’s (1998: 368) notion of reflective modification comes into play.

<sup>116</sup>Bach (2007: 40).

purposes of the semanticist project whenever I've assumed that e.g. Clause-indexicalism was committed to the claim that the material in the semantic clauses had to be taken as *internal* to linguistic representation (i.e. as part of the very content semantically expressed).

Well, I do not think RCM is guilty of a conflation that supposedly first lands us into (largely spurious) difficulties which are then taken to motivate *the minimalist turn*. The default position in semantics is still that meaning is given by *stating* syntax-determined truth-conditions and that a compositional semantics maps out the connections between expressions that generate those conditions.

As long as *this* remains the paradigm in semantics, it will follow that the stuff in the semantic clauses (the worldly conditions) *is* part of the content expressed in a context (that's what a tokening of a given sentence *picks out*)—or else, what would the MDP amounts to?

When I utter a true sentence, on the semanticist picture I am saying that the world is thus-and-so, I am giving a *precise* description of the world such that the sentence can be compared to the world and when things are *as the semanticist's clauses say they are*, then, *and only then*, will the sentence be true.

Accordingly, the suggestion that we ought to separate the theorist's posits from the question of speakers' cognitive access to those posits is, it seems to me, a red herring—sure, RCM itself is committed to a very similar idea, namely, that of enabling conditions which elude representation; but it is distinctive of RCM that there is no corresponding commitment to the idea that those conditions are *in any way* part of what a particular use of a sentence *says* (or determines).

Hence I reject Bach's (and others') dismissal of pragmaticist objections to the traditional conception of semantics (and its possible use against RCM as well).

## 6.6. Six Objections to RCM

What now of the positive part of the RCM proposal? How plausible is it? Let me voice some concerns that might well throw the tenability of RCM into doubt.

For a start, RCM seems to be flattening out a whole raft of good (if contentious) distinctions regarding different levels of content-“completeness” (corresponding to different conceptions of ‘what is said’).<sup>117</sup> We seem e.g. to have a rather firm intuition that “Tipper is

<sup>117</sup>The materials for an objection along these lines are to be found in the discussion in Bach (1994b: 268-9).

ready' is less complete than 'Tipper is ready for the hunt', for instance. If we deny that we *ever* approximate (let alone reach) a fully complete propositional content, it seems as if we lose perfectly workable distinctions of this kind (we should recognise, that is, that although each step in the completion process does introduce fresh ambiguities it also seems to remove pre-existing ones: *some* aspects of content are made more precise as the expansion process unfolds).

Secondly, we seem to lose a basic principle that is widely accepted in semantics, namely, that a difference in truth-conditions points out to some (potential, at least) difference in content. For, under RCM, differences in truth-value assignments seems relatively unconstrained by the semantics—it is up to the judgements of competent speakers, given the enabling conditions provided by the background, to assign a certain truth-value to a sentence (and those judgements are guided by the adoption of the supra-semantic notion of *understandings*: it is *this* notion that makes a difference to truth-value assignments).

It therefore looks, again, as if we are losing a perfectly good operative distinction that allowed us to individuate content fairly precisely (what mattered to a piece of content being the content it is was the difference it could make to the truth-conditions of expressions under embedment; there are no prospects, however, for a logic of understandings, given that the variety of transitions from content to truth-value assignment is indeed negotiated on case-by-case basis. It therefore looks as if *nothing* could account for intuitive differences in the content of expressions).<sup>118</sup>

Thirdly, there is something strange about saying that there are rules of language (or rules of inference) that do somehow determine (in a more or less loose sense) a level of content that, by RCM's lights, is *never* fully determinate. For there is a plausible principle, which Aristotle stated in e.g. *Nichomachean Ethics* (V, 10, 29-30) that "when the thing is indefinite, so is the rule". That is, the key idea behind RCM, that content is rudely minimal but nevertheless disciplined by rules, seems infected with incoherence.

Fourthly, it seems as if the truth-value fixing role of understand-

<sup>118</sup>There's a related objection, namely, roughly the same one that I invoked against Emma Borg back in chapter 2. It was raised by e.g. Blackburn (1986) against Perry's (1986) account and it concerns precisely the status of content in this sense: for Perry, it is often a propositional function, not yet truth-evaluable, while for Blackburn it is counterintuitive to credit thinkers with the thinking of general properties as characterised by propositional functions rather than specific fully truth-evaluable contents. I agree fully with Blackburn's point as *an objection against semanticism*. As far as RCM is concerned, though, my claim is that we do entertain thoughts that are themselves 'incomplete' but gain their (non-CET) specificity via embedment (we do not think skeletal matrices *awaiting* completion: we think thoughts that are as complete as they'll ever get).

ings will cause a proliferation of contradictions (or a slide into unrestrained relativism: truth is *always* relative to an understanding). In a given context (and keeping the facts external to speakers' judgement fixed), the same semantically expressed property (say, roundness) will be properly attributed and denied to the same object (say, a squash ball) by different speakers (given their different interest-relative understandings).<sup>119</sup>

Fifthly, to the extent that it denies that sentences have truth-conditions, RCM appears to amount to some form of content irrationalism. If so, it seems vulnerable to Boghossian's (1990: §III) charge of incoherence, since in making the claim that all sentences lack truth-conditions, RCM seems committed to the non-factuality of that very claim as well as to the attribution of truth-conditions to e.g. the semanticist's meaning theorems (or else, how could *those* theorems be *false*?).

The last and sixth objection is that it is not clear *whether* RCM is a *semantic* theory at all: by denying that we ever *entertain* fully propositional content, it seems as if RCM is saying that our attitudes are non-propositional, that our sentences never express (or communicate) propositional content. And it has long been argued (most famously in Lewis 1970: 190) that a semantics without truth-conditions is no semantics at all (and wasn't that a key part of RCM's *attack* on semanticism?).

### 6.6.1. Replies

Before I respond in detail to these objections, let me make clear the rather obvious fact that much of what I say here is little more than a promissory note. I needed to do a lot of work to motivate a move towards a radically minimalist position and I now lack the space for a full-blown exposition of RCM. Clearly, a thorough defence of RCM is something for future work. Here I can only indicate its likely direction.

The RCM reply to the first two objections is simple: we only have syntactic criteria for content individuation; the only constraints on interpretation that ought to be acknowledged are those that rule out *unavailable* interpretations rather than those that allegedly *determine* the mandated interpretation (even those constraints however have more limited reach than standardly assumed and they do vary as languages change, which suggests their privileged status is contingent

<sup>119</sup>Williamson (2009: 379-80) voices these concerns most forcefully.

and often enough a mere honorific).<sup>120</sup>

More specifically, while I'm happy to concede the *prima facie* plausibility of the remarks regarding the contrast between 'Tipper is ready' and its possible "completions", I suggest that further scrutiny should suggest that those remarks are not as well-grounded as it may seem at first.

Consider the move to 'Tipper is ready for the hunt'; this may indeed have made precise the *target* of Tipper's readiness. Are we any the wiser however with regard to what would count as being *ready*, given *that* target? Sufficiently trained? Properly groomed? Appropriately fed? We simply don't know. And the *appearance* of determinacy that *these* questions suggest is, I submit, only an appearance, for if we stop and ask e.g.: trained in *what* sense? Stamina? Olfactory prowess? Team discipline (trained to get along with the other dogs or with horses, or with strangers, whatever)?—the answer, again, will be that we just don't *know*.<sup>121</sup>

The initial impression of a difference in determinacy between the two sentences has thus turned out to be rather deceptive. And indeed the undercurrent of this thesis is that we should not derive conclusions about underlying structure from the *appearance* of determinacy, which in itself may only show that a practice is *settled*, and no more (it is the settled nature of a given practice with a term-in-context that makes questions about its sharpening both otiose and *unanswerable*—and if you think this conjunction is paradoxical, think again).<sup>122</sup>

So, it seems to me that the contrast between different sentences with respect to the presumed determinacy of their content ought to be taken *exactly* at (their syntactic) face value, and I am therefore not

<sup>120</sup> As I mentioned already, linguists have long drawn attention to what language cannot do. See e.g. Newmeyer (2005: §1.2.2), Hinzen (2006: 167), Pietroski (2005b: §2.1).

<sup>121</sup> Note that it is no reply to say 'ready for the hunt in some contextually salient way'. That's *not* what we mean, or say, when we entertain the thought-in-context, nor when we consider the expression in abstraction from its use-in-context—in fact, there's good reason to think abstractions of this kind are *entirely* artificial constructions (see again Taylor 1989: ch. 8 for discussion). Besides, why would *that* be an improvement over 'ready for some contextually salient activity in some contextually salient way'? Note that *each question we might raise as to the possible sharpenings of an expression* would add one more 'contextually salient' clause more or less *ad infinitum*. RCM does away with all the epicycles, which some might take to be an indication of its better fit with the phenomena.

<sup>122</sup> To see how much a practice can do for the appearance of determinacy consider the familiar difficulty that philosophers trained in contemporary analytic philosophy experience when tackling, say, medieval logic texts, or continental philosophy. Lack of familiarity *with the practice* of using certain terms in a certain way does make language opaque to the point of incomprehension for the uninitiated. And yet to the confident practitioners those texts are as crystalline as the jargon we've been browbeaten into accepting. Once again, familiarity breeds *content* (and *content* too).



sure that RCM is flattening out important distinctions bubbling underneath surface structure after all.<sup>123</sup>

Relatedly, the question of matching all variation in content as a *function* of variations in truth value cannot be uncoupled from the *need* for the deployment of speakers' judgement before content can be fixed as univocally as we can ever hope to achieve—the lesson from NS, I think, is that genuinely novel cases *are* problematic precisely because (and to the extent that) we do not (yet) have *grammaticalised* access to settled opinion on the matter. Horrific as the thought may sound, I contend that grammar (largely) recapitulates opinion.<sup>124</sup>

With regard to the third objection, the mistake here is in thinking of 'rule' in the terms made familiar within the semanticist framework. The point is, RCM rules are always provisional, open to negotiation and ongoing refinement, and so while the indefiniteness of the rules fully reflects the open-endedness of our concepts (and of our practices), that (in itself) doesn't lessen the normative import of those rules, or indeed their status *as* rules—they are the bearers of normative properties *precisely to the extent that they are parochially grounded* (the rails they lay down are wholly visible and it is we who tighten their bolts as new sections are *added*).<sup>125</sup> For we are part of a practice to the extent that we acknowledge the power of those rules: the contract we enter into as rational communicators is not negotiated with unseen and unreachable authorities but rather with our reflective peers.<sup>126</sup> This, I think, answers the Dummett Paradox above.

My view on these matters, then, is that we ought to replace the contractual view of meaning (whereby the compositional machinery enforces hard, unalterable rules to which we are at all times accountable) with the *contractualist* view, whereby meanings and rules are

<sup>123</sup>This goes of course against the revisionary tradition that stems at least from Frege and Russell's (1914/1993: 53) efforts to extract logical form from its linguistic "integuments", but so be it: I'm Chomskyanly minimalist at least in my admiration for the optimal elegance of NL surface structure.

<sup>124</sup>I'm being provocative here, but up to a point. Again, the literature on grammaticalisation contains pause-giving stuff for the semanticist. See e.g. Hopper and Traugott (1993/2003), Roberts and Roussou (2003), Traugott and Dasher (2005), Aarts (2007), Ariel (2008) and Muysken (2008). Even familiar claims about the relationship between linear and hierarchical structure in linguistics (e.g. Moro 2008: 203-04) heavily rely on *theoretical* prejudice against, say, claims that there *are* non-configurational languages. Note that even leading pragmaticists have what looks like a failure of nerve on this point. Even Recanati (2010: 117) talks of "special, deviant sense" and "marginal" cases/scenarios for NS-like strings. Zettl §393 is the antidote here.

<sup>125</sup>I'm here endorsing, modifying and expanding remarks repeatedly made in Travis (2006b).

<sup>126</sup>These ideas are fairly familiar from discussion in meta-ethics. It may sound heretic if not outright incoherent to claim that *the fabric of content* may equally be open to negotiation. My point is that it is of the essence of being the sort of reasoners we are that our concepts are in standing need of re-negotiation. It's the haze of use that again obscures this fact.

negotiable—they are on the same level as more general principles of morality and practical rationality. Much to Carnap's dismay, then, my claim is that in logic, as in language, there are *only* morals.<sup>127</sup>

At this point, the semanticist will recoil in horror and claim that this makes a mockery of the very idea of accountability and objectivity: without hard and fast linguistic rules our very thinking threatens to collapse into generalised incoherence.

My opponent will then insist that there *must* be fixed points in our language, that grammar *requires* Archimedean points, that not everything can be up for grabs (not everything *is* up for grabs, not everything can be put in the lexicon: practice-constraining structure surely belongs to grammar); it's already bad enough that we may lack external leverage in much of our practice, but language, of all things, language just *cannot* fall prey to this fate, the horrified reaction goes. In short, there must be a role for *syntax* (broadly conceived), if not for full-blooded semantics, and *that* is where objectivity resides (indeed, that is what makes language *possible*).<sup>128</sup>

I acknowledge the constraining role of syntax. I deny it reaches out as far as the semanticist (and on occasion, the Chomskyan too) thinks that it does, though. And I also deny that the lexicon/grammar division (i.e. the contrast between the idiosyncrasies of the lexicon and the compositional rules of the grammar, the nasty irregularities of the tokens and the reassuring regularities of the types) is as sharp as the semanticist thinks it is (and as indispensable as argued for at the theoretical level).<sup>129</sup>

Furthermore, I also insist that the search for objectivity, as traditionally conceived, is as misguided in language as it is elsewhere in philosophy: all we can have, even in language, is (Rortyeian) solidarity among epistemic peers, and we're much the better for it.<sup>130</sup>

The fact is, we can only be genuinely answerable to our peers; it is the semanticist idea that we *could* be answerable to the obscure doings

<sup>127</sup>Consider Grice's own view of the conversational maxims as something that regulates what *might* be said (in a deontic sense of 'might')—they are "desiderata [...] accepted by any rational discouser" (1981: 185). Discourse is deontically disciplined that is. And this was taken to regulate the level of post-semantic content. The (bold and I hope not foolish) idea behind the meaning-contractualism I endorse is instead that deontic criteria are *already operative at the level of content*. That's why I needed ExpNorm in my picture. On contractualism in ethics see e.g. Scanlon (1998: 5).

<sup>128</sup>See e.g. Hinzen (2006: 152-53, 167). I accept, of course I do, that there are constraints on interpretation imposed by syntax. But again the lessons from ch. 2 should be that those constraints are not as pervasive and as independent of our doings as the linguist routinely thinks.

<sup>129</sup>See Hudson (2007: 3-4) for some arguments against that division.

<sup>130</sup>See e.g. Rorty (1985).

of the compositional machinery that is defective (and too horrible to contemplate).<sup>131</sup>

Fear of parochialism, then, is simply misplaced, since *only parochialism can genuinely constrain our language*, and our logic too is much the richer for it.<sup>132</sup>

The problem, and it's an awkward one, is to give an account of parochialism *with a godly face*,<sup>133</sup> as it were, while resisting the temptation to try and incorporate parochialism into our machine and to think that we could have *semanticist* compositionality with a human face.

The machine and our parochial input, rather, are to be taken as *both* distinct *and* intertwined. If we are genuinely interested in the workings of language, then the *facts* about our competence demand that we view the machine as independent but only *as a function of* its *embedding within a wider normative practice*.

It is the machine's *relative* independence that generates radically minimal content: how that content gets assigned a truth value, however, is a matter (largely) for our judgement (MDP and UaKTC must go, that is).

Since the transition from content to truth-value assignments is essentially regulated by our judgements, it follows that *the structure of the machine itself must reflect our normative stance towards the world*: without that stance, the machine is blind.<sup>134</sup>

It also follows that the best theory of competence must indeed be structure-reflecting (just as the semanticist insisted), but the structure itself is in turn *judgement-reflecting*, for the steps along the route from sense to reference are not determined by the machine alone but are *essentially* guided by our judgements (our sensitivity to *particular* reasons-in-context).<sup>135</sup>

<sup>131</sup>I am not sure whether here I'm simply speaking differently, rather than arguing well. For Rorty (1989a: 7) with his Kuhnian hat on, speaking differently is all that is required for cultural change. I'm hoping I have also provided some arguments along the way

<sup>132</sup>Defences of parochialism can be found in Rorty (1985: 21), Gibbard (1990: ch. 10) and Travis (2006b: 107, 115). Specifically, Travis' suggestion is that the most a rule can do is give a prescription *under a parochial understanding of it*. I am aware of a seeming paradox in this area, for that prescription will strike us as reasonable only under an equally parochial understanding of 'reasonable'. Make of that what you will.

<sup>133</sup>A reversal and adaption of the title of Putnam (1990).

<sup>134</sup>What powers the machinery is the kind of rationality sketched in Scanlon (1998: 25).

<sup>135</sup>See also Sellars (1954: 340). This is a good place to stress again the difference between RCM and other forms of minimalism. Borg's view of content in effect is anything but minimal: for her, the semantic content of a sentence determines (and presumably *expresses*) a *large class* of possible states of affairs that may satisfy a sentence (by minimal means, content *represents* huge chunks of reality); Cappelen and Lepore's view is that we explain the apparent disconnection between minimal

The overall structure of the machine-*cum*-judgements is therefore *constitutive* of our ethical stance as agents-in-the-world under a standing obligation to renegotiate our epistemic position, including our semantic beliefs (and it is those *beliefs* that prime the machine).<sup>136</sup>

But what of the fourth objection, the charge that RCM will spawn contradictions at the drop of a ball (one and the same ball will both be round<sub>RCM</sub> and not round<sub>RCM</sub>)? Isn't a theory of meaning that dispenses contradictions with such abandon a *bad* theory, one that precipitates everything into triviality right from the start?

Well, in response, let me first note that in dealing with the Travis effect the semanticist alternative to RCM goes through an appeal to the character/content distinction.<sup>137</sup> The apparent contradictions are thus smoothed out by cutting content as finely as there are contexts of utterance/evaluation. I have already rejected this move at several places in this thesis. But as far as RCM is concerned, the point is that *there can be no assignment of truth value until an expression has been taken under a certain understanding*. In other words, the content of an expression, in and of itself, is *never* truth-evaluable, so it is a category mistake (!) to think there is a contradiction at all here (the enabling conditions are different, and so is the appropriate assignment of truth value).<sup>138</sup>

Similar considerations would apply to a related objection one could make, namely, that RCM faces a difficulty in giving a smooth account of bog-standard cases of ambiguity: when I think to myself "that's a nice bank", which minimal content am I entertaining, and is it different if I'm embedded in a river-scenario or in a financial institution-

semantic content and truth-value variation across contexts by appeal to speech act theory (with the result that we *say* false things a lot of the time); Soames' minimalism (especially in his (2005a) revision) is another form of CET, for he takes sentential content to be a propositional matrix awaiting completion-in-context. By contrast, RCM keeps content genuinely minimal at all times. *Whether* we say true things is for our judgements to establish. The compositional machinery is silent on *that*.

<sup>136</sup>I am pleased that even a Chomskyan like Newmeyer (2005: 116) acknowledges the limit of syntax in this respect.

<sup>137</sup>Williamson (2009), and the similar move in Predelli (2005).

<sup>138</sup>Williamson (2009: 379) constructs the problematic case for a version of RCM by recourse to a 'says that' clause exploiting Travis' contention that homophonic reporting is allowed for the Travis sentence (unlike, say, with indexicals): "This ball is round and Mary truly says that it isn't". The point however is that the mid-sentence switch in understandings blocks the conjunction introduction move. Clearly this makes the 'says that' operator a non-extensional one, which seems to require extensive *revision* of our practices—and wouldn't a contextualist account presupposing a difference in *content* between the properties ascribed in the two conjuncts be the obvious, most natural way of dealing with a case of this kind? Two quick remarks: all solutions have costs anyway; I'm happy to shoulder this particular one; quotation is messy anyway and quite often we read indirect quotation as being direct instead.

scenario? Clearly, if I say that there *is* a difference, I am embarking on the same path as the semanticist; if I say the entertained content is the same, I am defying common-sense.

Well, shocking as it may seem, the CRM reply is that I'm entertaining *exactly the same piece of content* in the two cases.<sup>139</sup> Expressions only acquire a meaning in virtue of a network of relations that connect them to other expressions, their causal histories, a community practice with them, their contextual embedment, our specific interests and so forth.

There is no surprise in the fact that the very same word acquires different meanings under different contextual embedments. We only ever learn words-in-context. There is no acquiring competence outside of a context. The ambiguity account presupposes not just that you can take words out of context but that you can also take context out of words; however, as Dorothy Parker would say,<sup>140</sup> *that* is something you can never really do.

According to RCM, then, the environment's contribution to content confers determinacy to an *attribution* of content without impinging *in any way at all* on the *semantic* profile of the expression.<sup>141</sup>

As for the fifth objection, Boghossian claims that any form of content irrealism (any view on which there is no straightforward deter-

<sup>139</sup>I suspect this is where RCM diverges sharply from Wittgenstein's later view of *meaning* (compare §139-140).

<sup>140</sup>Or should have said, rather.

<sup>141</sup>To repeat, the RCM view is that I entertain the word 'bank' *neutrally*, as it were. Consider a further example: on my way to the office, I walk by Inchdairnie Properties, a St Andrews estate agent. I glance at the sign. I take in, more or less unreflectively, the shop window, displaying the usual pictures of flats and houses. Do I even for a second entertain the thought that 'properties' might play the same role as in 'Cambridge Properties' when read in a metaphysics paper? I submit that in both cases we neutrally entertain the string 'properties' *and nothing else*. And astonishingly (I know), it's no different at the level of thought. Language is no more than a grid. And a very thin one at that. Embedment lays that grid down on things and with any luck we get where we need to go. Another objection needs addressing here: surely I cannot be serious in saying that the entertained content is the same in the two readings of 'bank'. For a start we can make sense of the claim that there *could* (indeed that there are) two readings available. Additionally, my view seems committed to the incredible claim that we entertain different content if we use words *for the same thing* from different languages (on the face of it, a rather extreme version of the Sapir-Whorf hypothesis). Am I then not confusing the door with the contents of the building? Isn't it more plausible to think that in entertaining semantic content we enter into a relation to a (perhaps Russellian) proposition, and if Salmon (2001) is right, isn't (something like) this actually necessary for the very possibility of language? Well, I stand my ground here. On the RCM view, propositions, or something like them, are essentially embedded entities. Sense is our way into them, but it only gains us access under embedment. That different entities get us to the same place is no objection to my account. The strangeness is, I contend, entirely an artefact of our training into model-theoretic accounts that pretend to incorporate the world into their very fabric. *Il n'y a pas de hors-texte*, honey.

mination of *worldly* conditions by the *form* of the entertained items of content) is terminally incoherent. My reply is that in taking it as a *given* that unless sentences determine truth-conditions there can be no such thing as content Boghossian is simply begging the question.

In fact, if we heed Evans (1976: 69), Boghossian's (and Lewis') claim must be strengthened to the requirement that a semantics give the *actual* truth conditions of sentences. And if the arguments of the previous chapters were any good, it seems clear that semanticism (and MDP in particular) does not provide such an account—it gives no *genuine* explanation of why an utterance of a sentence *is* true at a context. RCM's claim is that entertained content is *always* embedded content but sentences (and our thoughts) make only a minimal contribution to that content.<sup>142</sup>

There is no greater determinacy to the possible individuation of entertained content than what minimal disquotational means allow.<sup>143</sup> But that is not a problem for my account because *we only assess content from a context*. It is *that* which secures determinacy, and as I insisted already several times, that is something that eludes description and indeed theoretical systematisation.

There's more, however. Boghossian argues that the irrealist who endorses

(1) All sentences of the form "S has truth condition *p*" are false

is thereby committed to the Liar-like contradictory thought that no sentence (including (1), that is) has a truth condition. But instances of

<sup>142</sup>This is hard to put in clear terms. The idea is that there are two senses of 'entertain' at play here (but they do not coincide, not even modulo the necessary amendments, with the traditional notions of narrow and wide content). The content we entertain in the first, weaker sense is what we *actually* think (and disquotationally describe); by contrast, the *act* of entertaining the thought is environmentally embedded. So, it is the act of thinking it that *thickens* radically minimal content, but in this second sense it is not a sort of content that we *could* think (the thickening is carried out by the act of *thinking-in-context* but all along the object of thought remains minimal). So when I speak of entertained content what I mean is: radically minimal content *being entertained thickly*. I am not yet sure what RCM should say about false thoughts and *Scheingedanken*—how can mere thinking do the thickening in *those* cases?

<sup>143</sup>Here's another objection: we have (what we think of as) a well-understood grasp of quantification and, relatedly, of *different* levels of (semantic) abstraction. Compare '*x* is *F*', '*x* is hungry' and 'John is hungry'. If semantic content is *always* radically minimal, doesn't RCM flatten out all such distinctions? Well, on the contrary: it is the semanticist (and the pragmaticist) who let go of useful distinctions here, for often enough they maintain that NL sentences are mere matrices, propositional functions, and the like. For RCM, in contrast, those three strings are indeed different and express different thoughts, made determinate-in-context by various situated embeddings. It would take a rather long time to explain what RCM has to say about the notion of object-dependent thought, incidentally.

the quoted sentence in (1) can only be false, we are then told, if they *do* have truth conditions. Hence, content irrealism both attributes and denies the possession of truth conditions to sentences of that form.

RCM denies this is the case at all. Boghossian's objection confuses having warrant to deny that a sentence has truth conditions *as the notion is deployed by semanticists* with the semanticist requirement that without truth conditions there is no asserting that a sentence is false.

If I deny that the inferential rules for tonk are meaning-constitutive, if I deny that any sentence having tonk as its main connective does have a truth-condition, I need not presuppose that tonk *determines* a truth condition. Boghossian himself is on record as *denying* precisely this.<sup>144</sup> I doubt he would want to encourage the thought that he was thereby committed to a contradiction.

The fact is, it is perfectly coherent to deny that sentences have truth conditions *as standardly intended* while insisting that one has warrant for *that* denial.

On the final objection that RCM is no semantics at all: I borrow from Travis (2006a: 151) the ground rule that semantics ought to attribute to expressions semantic properties that expressions *do* have (rather than those fantasised about by over-eager theorists). If so, RCM *is* a semantics, to the extent that it respects attribution to expressions of the *minimal* properties that our best syntactic theory (once divested of its occasional excesses) says they have.<sup>145</sup> The thing that must be stabilised of course is the idea that we *can* individuate content finely enough without recourse to the traditional conception of truth-conditions. Again, that calls for further work, but I do not see in principle obstacles to achieving that goal.

There are remaining worries of course, in particular the fact that RCM appears to be but a (rather extreme) version of truth-relativism, with all the attendant and familiar problems.<sup>146</sup> Recall however that

<sup>144</sup>See e.g. Boghossian (2001, 2003).

<sup>145</sup>As my discussion of Chomsky's conception back in chapter 2 made clear, I agree with much of that framework but resist conclusions about the *definitive* status of sub-categorisation principles. On my view, syntax is both *more* and *less* autonomous of semantics than on Chomsky's view. It is even more detached from the world and from robustly conceived reference relations (for I deny the existence of the category of grammatical nonsense), but it is less autonomous than on his view to the extent that worldly influences have seeped into the priming of e.g.  $\theta$ -properties.

<sup>146</sup>How would RCM account for disagreement? Could it give an account in terms of *cognitive shortcoming*—as urged e.g. by Wright (1992: 14)? The difficulty is that RCM seems forced to say that when opinions clash we disagree not about content but about whether the enabling conditions justify assertion of a certain minimal piece of content. Trouble is, we lack the means so much as to *describe* those conditions; we lack, that is, the very means to spell out the details of the disagreement

RCM's claim is that convergence in understanding is an *enabling* condition, not something that is part of the machinery itself—we *assume* convergence and we then assign truth values absolutely. I therefore do not think that relativistic worries are justified here—on the RCM view, the standpoint is not agent-relative: it is *convergence-relative*, and that's the best shot we can take at objectivity.<sup>147</sup>

### 6.7. Conclusion

Let me now add a few more details regarding the RCM proposal.

We started off with the appealing thought that our self-constitution as rational agents seems to require an account in terms of rules rather than of habits. Language is, clearly enough, the distinguished home of our rationality, and our difficulty was to locate a rule-based account for it that would make its normative hold on our practices sufficiently detached from best opinion to make it capable of guiding us in appropriate ways, without doing violence either to the *known* facts about language itself (its relative structural independence, its internal laws), or to intuitively desirable constraints on our epistemic access to those rules.

Over the previous chapters, I have assembled what I think are good reasons to argue that the compositional rules fall short of full determination for both meaning and truth-conditions. An account that incorporated our responses (our judgements as to the obtaining of those conditions) *right in the driving seat* appeared just as problematic, for it is unclear how we could secure the required *a priori* connections between the judgements of ideal speakers and the truth of those judgements, given the elusiveness of the required idealisation.

The difficulty then was that on neither account do we have a vin-

(and hence for mapping the road to conflict-resolution). The difficulty is sharp but I think there is space for saying that our sensitivity to *pro tanto* reasons-in-context tracks the obtaining of the appropriate enabling conditions in a manner that although not amenable to theoretical description is nevertheless *answerable* to peer-review. The shortcoming is therefore eminently *ethical*—the guilty party failed to detect properties that resist formalisation but that we are nonetheless under a standing obligation to respond to. The improvement over semanticism is that under RCM room is made for our tinkering with the compositional machinery (we are entitled to *challenge* settled opinion as encased in the rules of grammar).

<sup>147</sup>Skorupski's (1985; 2010) work on the notion of convergence provides the beginning of an account in this direction. If you want to call my position semantic irrealism, I won't be offended (*contra* Wiggins (1987), I don't accept that convergence *has to be* the mark of truth). Of course, I concede that more work is needed to stabilise the view, but in this dissertation my main concern is with providing motives to move *away* from the CET-influenced picture. I am still working on making RCM a *compelling* view in its own right (and to myself in the first place).



dication of our practices (let alone their justification), for on the Fregean account we posit inflexible rules that supposedly map sloppy expressions to sharply defined concepts, whereas on the alternative account the circumstances of our agreement in judgements are left just as mysterious as the Platonistic epistemology it was purporting to leave behind.<sup>148</sup> The mechanisms that are supposedly ensuring agreement and tracking in the respective accounts, that is, seem to be left floating in an epistemic void.

In response to these difficulties, the basic idea behind RCM is that we should move away from a conception of the compositional machinery as presiding over a calculus of *standing reasons* based on rules (with rules defined as unerring Fregean functions beyond our epistemic reach) to a conception whereby what the machinery disciplines is, rather, a calculus of *entrenched reasons* (those generated by logic and semantics *qua* Kuhnianly normal disciplines).

The traditional contractual picture of meaning whereby we delegate the task of determining complex meanings to compositional devices should therefore be replaced with a *contractualist* view, where the commitments we incur with respect to atomic meanings are only provisional and do not as yet suffice to *force* a corresponding commitment with respect to complex meanings.<sup>149</sup>

As we confront *genuinely new* complex meanings, the old lexical meanings have to be revised, and on occasion even our conceptual scheme may need revising—in effect, a new concept comes to be determined as we grasp new content *that wasn't contained in the original stipulations for the atoms*.

Moreover, the gradience from the grammar to the lexicon is never sharp, but, again, is always provisional, for revisions to meanings and concepts will also force changes in grammatical categories and hence in the compositional machinery itself.<sup>150</sup>

In short, CC has engendered an illusion of productive understanding that we must be rid of: if understanding is knowledge of truth-conditions robustly conceived, then we do *not* understand genuinely novel sentences. And when we do understand them, we do not do so from the bottom up anyway. At best, PoC explains judgements

<sup>148</sup>I'm simplifying horrendously here, of course. But Wright's (2007) most recent reflections on these matters are remarkably honest in the assessment of the difficulties in stabilising an RD account in this area.

<sup>149</sup>Does this not concede that the machinery *did* determine complex meanings on the basis of the *old* meanings? Here I can only answer: yes and no.

<sup>150</sup>See again footnote 141 ch. 2.

regarding *settled* meanings.<sup>151</sup>

My account does incorporate opinion, but not as an *a priori* constraint on meaning-determination. It is, so to speak, *structure-inflected* opinion that can properly be said to determine extension-under-embedding.<sup>152</sup> This, I think answers the EP and illuminates the CC/PoC connection in a manner that is more respectful of the facts than semanticism.

But what of the RFC challenge to the very idea that we *can* interpret signs without getting caught in a vicious regress? Well, in tacitly agreeing on what counts as a *reasonable* interpretation-in-context, it is correct to say that we go beyond the evidence-in-context (linguistic and worldly), for countless alternative interpretations are available, given the facts and dispositions in that context.

In settling on what counts as reasonable, in that context, it might then seem that we fall prey to mere habit rather than engaging in a rational response to properly salient rules. But in fact, what we do do is *properly* (i.e. rationally) *ignore alternative interpretations*.<sup>153</sup>

So, the blindness in our basic reasoning, our blind submission to rules of language that we cannot raise to consciousness on pain of paradox, remains nevertheless fully rational because, contrary to a suggestion in Pettit (2002b: 4), it is not due to lack of imagination—it is most resolutely not a *cognitive* failure.

Wittgensteinian blind obedience to the rules, rather, is properly seen as a *semantic blindsight* phenomenon: interpretive choices that are unwarranted by our embedding go *unseen*.<sup>154</sup>

Given the facts about our contextual embedding, that is, doubts

<sup>151</sup>But even there, the empirical evidence is inconclusive. Some results seem to indicate a difference in processing speed between idioms and compositionally-generated meanings (Ariel 2008: 208–09). Other results suggest we seldom make recourse to compositional rules in parsing meanings. See the already-cited Dąbrowska (2004: ch. 2).

<sup>152</sup>To adapt Evans (1982: 209, fn. 7), best opinion is driven by *structured responses to situations*.

<sup>153</sup>Chomsky (1986: 263–264) draws a fascinating parallel between the reliance on background assumptions that is built into our judgements about moving objects (on observing a head turning to face us in the distance, the visual evidence is consistent with the hypothesis that the person has changed shape, not that she has turned round) and a parallel dependency in the case of judgements about sentential *meanings*. It is a network of background assumptions that secures stability for our meanings just as it does for our perceptual judgements.

<sup>154</sup>Am I sliding into a form of Relevance Theory here? I don't think so, at least in the sense that I am not positing determinate candidate *interpretations* there for the taking. Properly speaking, on the RCM view our expressions are *not* interpreted at all. As soon as interpretive doubt is raised, there is no possibility of neutralising it—the evidence is *always* compatible with indefinitely many readings (and the standard Chomskyan invocation of nativism merely pushes the problem one step back). Note a double sense of blindsight here: enabling conditions are *seen* (Luntley 2003: 50) and responded to, while we are *desensitised* to blind semantic alleys.

concerning interpretation cannot even arise, because it is that embedment (and nothing else) that has sharpened content and neutralised alternative readings of the expressions—those readings *could not even be* salient.<sup>155</sup>

Now, this is of course all too quick, but there are two important points to note here.

First, the blocking of alternatives is only temporary, and it is rational inasmuch as the set of Exp-norms in force in the practice as a whole is itself temporary (it is amenable to self-correction at any time, at any node in the belief system).<sup>156</sup>

Secondly, we are confined within minimal content at all times (there's nothing, or at least very little, beyond disquotation). Language, that is, generates *genuinely* and *irreducibly* skeletal content and it is our situatedness that makes it as determinate as it'll ever get.

In other words, there is in our practices an *illusion* of determinacy which, given the circumstances, is entirely harmless; harm (and vicious paradox) ensues if we *try* to make content determinate.<sup>157</sup>

Our thinking operates, in essence, on radically minimal pieces of content. But we are fully rational in treating them *as if they were* fully determinate thoughts because it is facts about our embedment that secure whatever determinacy can be achieved for them— here's another slogan: *no identity without embedment*.<sup>158</sup>

<sup>155</sup>Here I disagree with the otherwise penetrating analysis in Staten (1984: 105) that in “yielding to the automatism of language” as we follow rules we are also “multiplying possibilities” and “creating alternatives” (that's, I suppose, an allusion to the intrinsic polysemy of the sign that so exercises Derrida 1972: 2). What matters, rather, is our *rightful blocking off* of interpretative side-roads. And to answer McDowell's (1987: 96) question regarding the *right* with which one ignore alternatives: there is *no one story* to tell about that.

<sup>156</sup>Just as Sellars (1956: 170) had famously taught us. Lance and O'Leary-Hawthorne (1997: 131ff.) develop the theme further. It is a delicate question how far we can push the analogy with other enterprises. An obvious analogy is with positions in metaethics such as Scanlon (1998), Gibbard (1990) and Skorupski (2010). The difficulty is to imagine what, say, *radical* contractualism about semantics would *really* amount to. Skorupski's (1996: 73) definition of *convergence* as the mutual recognition by epistemic peers of the rationality of one's commitments is promising, but there remain lingering doubts as to whether an approach of this kind can get the explanatory priority right. As I say in the text, the truth of the matter is more likely to be that *there is no single source* for the authority of language over us.

<sup>157</sup>My earlier talk of alternative readings may have suggested that determinacy of content was waiting in the wings, as it were. That, I think, is a temptation to be resisted, or else we end up making this proposal just as incoherent as the two alternatives from which we started. It is not that we have fully complete interpretations awaiting selection. That is precisely what the RFC forbid. *All* candidate pieces of content are incomplete. Again, it is our situatedness that bestows as much determinacy as they'll ever get.

<sup>158</sup>Undoubtedly, I'm here sympathetic to the anti-representationalist strain in e.g. Taylor (1980) and much of Rorty, in particular, I suppose, his (1989a). But Chomsky too is deeply anti-

Unless I am mistaken, it seems to me that this qualifies as a form of meaning rationalism in the sense sketched above. We are sensitive and answerable to reasons which are made precise (and normatively relevant) by facts about our situatedness that do escape systematisation (those facts are *enabling* conditions for our thinking, they cannot be represented in our thinking, nor can they be captured theoretically).<sup>159</sup>

Accordingly, our rationality is fully preserved even in the face of apparently blind reasoning *precisely because we display sensitivity to reasons which are made salient only by facts about our situatedness*.<sup>160</sup>

And so the norms of meaning-expectation that I sketched in section 6.2.3 with respect to reasons-meanings pairings are themselves (partly) grounded in facts about our very situatedness. Given those (ineffable) facts, the rational thing to do is to issue certain kinds of judgement. This, I think, answers the RFC paradox (as best as we can, at least).

Note however that judgements thus grounded are not extension-determining properly speaking. Rather, they reflect *facts* about extensions that are (partly) determined by the practice as a whole (by our rationally taking our embedment as *simultaneously* content-fixing and reasons-generating in the appropriate way).<sup>161</sup>

One might nonetheless complain that this proposal seems to leave largely untouched the greater puzzle about language: *its* ability to point in indefinitely many directions and *our* ability rationally to select the correct direction-in-context without having to give thanks to

representationalist with respect to the properties of expressions. I like to think that RCM (but not Chomsky) does take the full measure of that stance.

<sup>159</sup>Here there are points of contact with Searle (1980, 1983a), Glendinning (1998: ch. 8) and Dancy (2004: ch. 11.2).

<sup>160</sup>The RCM picture here is that in asserting the minimal content (I won't call it a proposition, because strictly speaking we are not asserting a proposition), we are saying that *there are enough grounds available in the context* for us to commit ourselves to the truth of the statement made by our use of a certain sentence. The crucial point is that neither the sentence nor the context *uniquely* determine which grounds will warrant assertion. What makes one judgement more justified than other, then? Sadly, neither we nor our theories can provide an answer to that. Am I ducking out of the issue? Isn't semanticism explanatorily superior to RCM in that respect? No, on either count. There is *no one answer* to that question. And semanticism misrepresents the facts grounding agreement by pretending that there is. Is RCM-sensitivity-to-reasons-in-context then a form of semantic epistemicism—didn't after all Williamson (2000: 180) speak of epistemic agents as "causally sensitive" to evidence beyond their direct grasp? Well, it is and it isn't. We *draw* boundaries that cannot be *given* (*Investigations* §68). See also Richard (2008: 3).

<sup>161</sup>In a sense, the role of our *decisions* is thus only *partially* extension-determining. Here there are points of contact with Rayo (2008).

the Deity for our subsequent agreement in judgement.<sup>162</sup>

Well, much as I dislike hand-waving talk of ‘forms of life’, and much as I wish there was a conceptual analysis account available in this area to keep us all honest, I cannot see a more promising way of accounting for our blind acceptance of judgements about semantic properties than the one I have just sketched in terms of sensitivity to reasons-in-context. The puzzle, it seems to me, only persists if we examine language in isolation, as if it were, precisely, a formal object—but then, even formal objects are objects-in-context anyway.<sup>163</sup>

The claim I’ve been labouring towards, then, is that while there *are* normative conditions attaching to our expressions, they are always provisional and largely parasitic on background conditions that elude articulation. Accordingly, the judgements about their satisfaction that we routinely make are, although fully rational, essentially *pro tanto* and *pro tem*.

In that sense, it is probably correct to say that we do indeed delegate authority to the compositional machinery, but only with respect to those selections that we have no *reason* (or no *need*) to question.<sup>164</sup> It is rational to project in the standard cases, that is, because those cases have *already* been tested (and if need be, adjusted for). In genuinely novel cases, however, our judgements play a different, extension-determining role, for the selectional properties of the atoms are not fully determined until our judgements reset the machinery appropriately.

The Extension Problem, then, has no single solution, but rather a whole array of them, which is what we should have expected all along because the authority of language over us has no single source either but, again, is drawn from a multitude of facts about our embedment.<sup>165</sup>

<sup>162</sup> *PI* §234.

<sup>163</sup> And just as enabling conditions resist *explicit* theoretical treatment, so does agreement—they both are *presuppositions* for meaningfulness (*Zettel* §430). But again, RCM departs from Chomskyan orthodoxy in thinking that the agreement is grounded in *anthropological* (and deeply normative) facts and not biological ones.

<sup>164</sup> This is where I strongly disagree with Chomsky’s idea that deviant sentences *require* an interpretive effort, whereas standard sentences *impose* an interpretation *irrespective of our judgements*. This confuses semantic *entrenchment* with the presumed facts of ‘natural’ necessity, as the Chomskyan like to put it—for example, Boeckx (2006: 4), Hinzen (2007: 27). There are of course several areas of difficulty left unaddressed on my account. Why, for instance, should the RFC not apply to the judgement that, in a particular case, the PoC-driven selection is indeed a tried-and-tested one?

<sup>165</sup> To return to the quote from Carnap (1942: §7) I discussed back in §3.2, p. 65, according to RCM what sentences provide as a starting point for evaluation is a *search procedure*—my preferred formal account of this is, near enough, of the kind given in Crouch and van Genabith (1999), Dal-

And this too is what we should have expected all along, because the reality confronting our epistemic efforts has an essentially *dispersed* nature<sup>166</sup>—it was our compulsive craving for generality (and our acquiescence in “the deeply ingrained worship of tidy-looking dichotomies”) that made us overlook that.<sup>167</sup>

The title of this dissertation leaves it open that there could be many routes to and from sense. This is as intended. The account we need is one that does justice to the many ways in which words are used, the many ways in which reference is secured, the many ways in which our skeletal thoughts are made true under contextual embedment.<sup>168</sup>

The long-suffering reader will however demand that now I give the truth-conditions, under my account, for a sentence like ‘John cut the sun’ (or ‘this chair is brown’ for that matter).

Well, here RCM departs from e.g. Borg’s (2004: 236) minimal conception of *liberal* truth-conditions that semantically expressed sets of conditions admit of satisfaction by a wide class of states of affairs. For on that account *it is part of the content* of the expression that there be claimed to be *the* cutting relation holding between John and the sun.

There is, of course, no such *unique* relation. The semantic content of that sentence is exactly its face-value content, disquotationally minimal, and no more. Try as we may, we could never specify that relation, and the problem is not merely one concerning the *limitative powers* of the language (of *any* language), but rather it is a problem

rymple (2001: ch. 9) and Kempson *et al.* (2001: ch. 3). Unlike these authors, however, I don’t see the online interpretive search as the progressive *refinement* of content (all of these approaches still fall within the scope of my CET label). Moreover, I’m quite happy to contemplate *empty* searches too: just as nonsense showed, we can entertain perfectly well-formed semantic content that bears no representational/referential import. In case there are any doubts left, I’m drastically internalist about content while remaining sanely externalist about our practices. The thing is, unlike e.g. Stalnaker (2008: 135)—and any other wholesale (and wholesome) externalist—I do not think that our causal interactions with the world determine meaning and content. That, I suggest, misconstrues what embedment *is*, and what it *does* to us. Properly speaking, our words do not reach out all the way to the world, and neither does the world reach all the way to us (content is two-way non-luminous, that is).

<sup>166</sup> As Edwards (1990: 232) happily put it.

<sup>167</sup> Wittgenstein (1933-35/1958: 17), Austin (1962: 3).

<sup>168</sup> Again go back to Dunbar (2005) for some lessons about the semantics of compound nouns. The priming and compositional processing of the semantic features of the atoms for the most basic Merge operations is largely judgement-dependent. It doesn’t follow that it is *always* so (anaphora imposes rather strict constraints on interpretation, for instance). And the upshot is: if we cannot give a *unitary* semantics for meaning-composition, the insistence that complex meanings are nonetheless compositionally determined is just sheer dogma. And if you (still) think compound nouns and nonsense are *marginal* cases, consider the problems that prepositions pose to PoC (see e.g. Taylor 1989: §8.3).

about the metaphysics of properties (there is indeterminacy all the way down) and content.<sup>169</sup>

It is therefore only facts about our embedment that will make it rational for us to judge that, given the obtaining of certain other facts, John did indeed cut the sun. Those facts, as we have seen, are only partially about our responses, though.<sup>170</sup>

<sup>169</sup>I thus reverse Williamson's (2003a: 706) judgement and say that the determinacy assumption embodied in CET and DETL derives linguistic conclusions from ontological prejudice.

<sup>170</sup>There are of course lots of loose ends in my account. Let me say a few very quick things here. First, why should we take my account of semantic blindsight as the proper ignoring of alternatives to be any improvement over the Relevance Theory (RT) story? On that picture we are at least given some constraints on interpretation (e.g. the Principle of Relevance) and an explanation of why speakers home in on a unique interpretive choice. In contrast, to isolate content I merely offered generic (and mysterious) talk of sensitivity-to-reasons-in-context. Well, as I see it, two things favour RCM: doing away with the problematic assumption of content determinacy removes the incoherence that dogged RT; further, the phenomenologically implausible RT claim that content is *always* secured inferentially is also removed (on the RCM story, grasp of semantic content is immediate, indeed quasi-perceptual). Secondly, what does RCM have to say about NS? Well, RCM views syntax as *radically* independent of *reference*. We understand NS *minimally*—grasp of NS content is *exactly the same as grasp of so-called normal sentences*; what we lack is enabling conditions for NS, but in that respect Chesterton's *The Man Who Was Thursday* or Charlie Mingus' *Tonight at Noon* are no odder than the Monty Python cheese shop that doesn't sell cheese, Tommy Cooper's corner shop that sells corners, or Jack Benny's cat burglar who stole 15 cats last week. RCM, in short, does away with the MDP and with the untenable commitment to the semanticist claim that meaning reaches out all the way to the world. Thirdly, what of the CR? What does RCM say about that? The reply is that RCM denies there is such a thing as a domain-neutral CR. Just as the radical Quine insisted, all logical principles are amenable to revision: moreover, there is *no theoretical gain* in imposing a CR that would hold (even only provisionally) everywhere. Each domain of enquiry will impose its own CR and each CR will be incommensurable to the others—logic is the *many sciences* of reasoning (I think Sainsbury 2002a has indirectly answered the doubts about radical Quinianism raised in Wright 1986b and incommensurability should take care of the monistic critique of pluralism in Priest 2006: ch. 12 and Read 2006). Finally, note that RCM-semantic, like cognition generally (Williamson 2000: 180), is indeed a highly *opportunistic* enterprise (Uriagereka 2008: xvi)—but *contra* Putnam, it's *not* sloppy: there is no one single process that secures semantic content; content-determination is a complex process involving several faculties *even with respect to allegedly pure semantic content*; whilst there is still space for grasp of structure under RCM, its significance is toned down, since structure itself is seen as the grammaticalisation of past pragmatic input (see e.g. Kay 1997: 4) and its grasp in any case relies essentially on *awareness* of ungraspable enabling conditions: sensitivity to those reasons is *constitutive* of our being agents (reasoners, thinkers, selves). It's not just that language does a lot more than communicate information (Gibbard 1990: 3). It's that *the way in which it carries information* reflects that extra-communicative (reason-constitutive) function (Valin 2001: 321). That's why RCM is a proper part of meta-ethics: you do not place yourself appropriately as an agent in the world until you see the world properly in terms of responsibly responding to reasons-in-context for the making of certain (reflective and unreflective) judgements regarding the apt employment of sentences. Syntactico-semantic structure is thus ineliminably *ethical* to the extent that it *requires* at all times our validating judgement (under the constraints above) as to *which* representational claim is being made and as to *whether* it is correctly judged to fit the facts. I think this retention of a modified UaGS by RCM should address

What we require, then, is a dappled semantics that perfectly matches a dappled world.<sup>171</sup>

Dummett's (1989: 181-83) powerful objection against Wittgenstenian accounts of meaning. It also ensures that the facts directly attributable to semantic competence fully *rationalise* our semantic beliefs.

<sup>171</sup>See Cartwright (1999). A summary of my thesis can also be found at §81 of the *Investigations*.









## List of Abbreviations

CC	Creativity Considerations	p. 11
CET	Content Expansion Thesis	p. 75
CL	Classical Logic	p. 100
CMP	Chomskyan Minimalist Programme	p. 122
CR	Consequence Relation	p. 139
C-sentence	Chomsky-sentence (“colorless green ideas...”)	p. 31
DETL	Determinacy Thesis for Logic	p. 139
ECE	Epistemic Constraint on Explanation	p. 18
EP	Extension Problem	p. 13
EPoC	Epistemic Principle of Compositionality	p. 14
ExpNorm	Expectation Norms of Meaning	p. 172
EPU	Epistemic Conception of Understanding	p. 12
GCI	Generalised Conventional Implicature	p. 111
GP	Generative Principle	p. 14
LCB	The (genuinely) logical connectives are Boolean	p. 101
LF	Logical Form	p. 71
LF-Indexicalism/LFI	Logical Form Indexicalism	p. 78
LIR	Logic as Idealised Reasoning	p. 147
LSR	Logic is the Science of Reasoning	p. 134
MDP	Meaning Determination Principle	p. 18
MSA	Multiple Senses Account	p. 104
NL	Natural Language	p. 1
NLB	NL connectives are Boolean	p. 100
NS	Nonsensical Sentences	p. 22
NT	(Meaning) Normativity Thesis	p. 169
PI	<i>Philosophical Investigations</i>	
PoC	Principle of Compositionality	p. 14
PoCU	Principle of the Compositionality of Understanding	p. 15
RCM	Radical Content Minimalism	p. 161
RD	Response-Dependence	p. 177
RFC	Rule-following Considerations	p. 168
RPA	Radical Pragmaticist Argument	p. 70
SD I	Semanticist Dilemma I (learnability)	p. 21
SD II	Semanticist Dilemma II (understanding)	p. 24
SD III	Semanticist Dilemma III (building-blocks)	p. 44
SD IV	Semanticist Dilemma IV (worldly knowledge)	p. 53
SD V	Semanticist Dilemma V (CET)	p. 86
SD VI	Semanticist Dilemma VI (MSA/SSA)	p. 102
SD VII	Semanticist Dilemma VII (Scope Problem)	p. 103
SD VIII	Semanticist Dilemma VIII (CET/LSR connection)	p. 160
SP	The Scope Principle	p. 114
SSA	Single Sense Account	p. 110
TCP	Truth-conditional Pragmatics	p. 71, fn. 28
TD	Thoughts are Determinate	p. 186
UaGS	Understanding as Grasp of Structure	p. 16
UaKTC	Understanding as Knowledge of Truth-conditions	p. 24
USC	Underdetermination of Semantic Content	p. 70



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